VOL. 32, No. 4 July, 1925

THE PSYCHOLOGICAL REVIEW

PERSISTENT PROBLEMS IN SYSTEMATIC PSYCHOLOGY. II. THE PSYCHOLOGICAL DATUM

BY RAYMOND HOLDER WHEELER

University of Oregon

The problem of the psychological datum cannot be discussed adequately without reference to epistemology any more than the conventional definition of psychology can be discussed without reference to metaphysical problems. The reason for both situations is the circumstance that neither definition was formulated in such a way as to avoid concessions to philosophy. Two artificial distinctions should be cleared away in an attempt to answer the question: What is the psychological datum? We have already dealt with the first of these, namely, the distinction, on metaphysical grounds, between mind and body, a distinction which parallelism and the double-aspect theory concede, logically, as much as the old interactionistic dualism. This paper deals with the second artificial distinction, that between process and meaning, and its effect upon our notion of the psychological datum. Like its metaphysical correlates the systematic distinction between what is existential and what is not accepts the situation which it attempts to avoid. In order to show a likeness between this and the epistemological distinction between subject and object we shall present the problem, first, in its epistemological terms.

I

An experience and its recognition are two different processes. Stated more concretely, to feel or to live a pain

is sentience and an awareness of pain is consciousness—a cognition.

An infant has colic for the first time. Obviously he feels, but he does not know what or that he feels. He feels, but, let us say, is not conscious of anything. Likewise he hears for the first time, but does not know what or that he hears. He recognizes nothing. Now consciousness depends upon the so-called subject-object relation. When we say 'consciousness of pain' we mean that pain is the object of which a recognition process is the subject. To admit that the infant knows the pain or the object producing it assumes that he can introspect or observe, and further, it assumes the presence of associative processes none of which are as yet available. On the other hand, it is quite certain that the infant is not anesthetic.

We are forced to the conclusions that there is a type of mental process devoid of a cognitive feature and that, with respect to a form of mental life which contains this cognitive element, the former is subconscious. These considerations lead to the further assertion that living or having mental processes can be distinguished from experiencing meanings and that this difference assumes either the form of a distinction between introspecting and not introspecting, or the difference between analyzing and not analyzing a situation. Since both types of processes are mental we may justifiably classify these two realms as the subconscious and the conscious, or as the conscious and the self-conscious.

Smith and Jones are engaged in an argument. They are driving to a distant town by automobile and arrive at a fork in the road. Smith suggests taking the right highway. "No," Jones insists, "take the left." Whereupon both feel apprehensive concerning the remainder of their journey. None of us would deny that Smith and Jones are conscious of a certain situation. Both are analyzing, choosing, and recognizing a dilemma. On the other hand, each may be visualizing his destination without having the faintest idea that he is doing so. Neither need realize that he is experiencing meanings. Instead of listening to what the other

had to say, either might have reflected that he had just spoken; he might have recognized his apprehension, or his friend's persistence. Meanings which were implicit in his reaction at the time might have become explicit in a recognition of them, and other meanings might have been derived from those already developed. All of this, however, did not happen. Other and different processes claimed their attention and they went on. Smith and Jones were doing certain things without knowing at the time what they were doing. They did not pass on to the required stage of cognition. With respect to that knowledge or knowing without which they would have been oblivious to their performances, Smith and Jones were having subconscious mental activities. While these activities involved reflection, they lacked a reflection upon the reflection. In the same sense that it is possible to feel pain without recognizing the pain, it is possible to recognize without recognizing the recognition. But this is not the end. As long as the reflection on the reflection is not in turn subject to reflection it is just as unknown as the first one. Without a final act of recognizing, even the introspective or self-consciousness, or the deliberate elaboration of a meaning, is subconscious in the same way and for the same reason that the infant's first pain is subconscious. Associative processes and past experiences are not brought to bear upon the preceding mental step.

The Freudians have every logical reason for including any number of wishes, ideas, feelings, reflections and what not in their subconscious, for it consists of any mental process which, because of an inhibition of the final reflection, fails to become a datum of consciousness. And it need not be argued that any one of these subconscious processes may be as highly conscious as we suppose the *real* conscious to be.

However foolish all of this may sound, the situation is a serious one. To recognize the vicious circle is relatively easy. To say that the conscious or known is unconscious or unknown until the known becomes known sounds like nonsense. It is not so easy, however, to offer an adequate substitute for this reasoning, and a neglect of the issue means

XUM

that a genuinely psychological problem will never be solved—the difference between process and meaning.

II

To attack the thesis in its own terminology, an idea and one's consciousness of the idea are the same. If they are different, it is not the idea of which we are conscious but the consciousness of the idea! The idea is never conscious; only the reflection that follows; not even that, but only the next, and so on *ad infinitum*. We are thus never aware of anything and are confined to that realm of oblivion on the borderline between subject and object—a realm which is neither one or the other.

The better course to pursue is to avoid the subject-object problem, to cast it aside as epistemological and not psychological, and in its place, to accept things as we find them without bothering about the question: How do we cognize? Mind cannot solve itself. Thus we assume that mental processes are existential and that by abstraction we resolve percepts into their constituent elements. The material of psychological science is first-hand experience. We take for granted that we can differentiate between what is existential and what is not by having recourse to a distinction between two kinds of experience, immediate and derived, the first leading to description and the second to interpretation —the one to a statement of content and the other to meaning.

In other words, the datum is not to be regarded systematically as a cognition but as a given. Psychology does not deal with ideas-of but with ideas. Recall that we have attempted to avoid a problem of knowing; have we succeeded?

III

Do we mean by existentiality merely that a mental process exists for us? If so, we are assuming (1) that its existence and its observation are two different processes, or (2) that its existence and its observation are the same, or (3) that we can ignore the fact of observation and consider the process as fact, not caring about assumptions (1) and

(2). In any event we are assuming the observation. Can we accept position (3) and take for granted the observation without inquiring into its probable nature from a psychological standpoint? Certainly not so long as we are vitally concerned with its adequacy. Moreover, we are concerned with the nature of observation itself when we differentiate between description and statement of meaning. We are admitting that the process of observing is dependent upon attitude, or in other words, upon particular groups of associative processes by means of which the product is in a measure determined. We are implicitly accepting the definition that observation is cognition. We find what we are prepared to find by determining factors. Moreover, we made a deliberate admission of the problem of knowing in a separation of process from meaning only to attempt a neglect of that problem in the concept of existentiality. By what right have we done this when we have already admitted that the process of observing affects the thing observed? Obviously on the ground that, for practical purposes, we may suppose the thing observed and the process of observing—subject and object—to be the same. Thus we slip over into position (2). (We have already dispensed with position (1).) And so we assume that a mental process is after all a cognition in which subject and object are one. But now we have on our hands an inseparable unity. We cannot conceive of a complex state, for in so doing we admit analyzability and the separation of subject from object. Moreover, we have assumed that all existing mental processes are known and we have proved too much, for we have said that no mental process exists of which we are not aware—an assertion that will not admit of proof. Conversely, we cannot demonstrate that a mental process exists of which we are not conscious. We are now in another difficulty. How are we to reason from one datum to another with subject tied up in the object? We are making consciousness a mosaic when we regard the datum as a given defined as above.

Positing a datum which by implication includes subject and object means, further, that we are not making any

XUM

observation at all, or perhaps, we are doing nothing but make observations with nothing to observe. In any event we are in the same dilemma as in the thesis—neither conscious nor subconscious! We conclude, therefore, that the theory of existentiality does not avoid the problem of knowing, but accepts it in one of its most vicious forms. When we make subject and object one, the unity partakes of neither. The consequences are the same as in the thesis when subject and object were segregated by a logical gap. *In other words, it is illogical to separate description from interpretation and it is equally as illogical to make them one.*

Now it may be contended that the given is merely that which stands up under observation—that which comes to us as fact. It is unfair to drag in the problem of knowing because the process of knowing is a constant and therefore negligible. Let us see if this is true. What is it that stands up under observation introspectively? What is the given? Here is but the beginning of a long list: sensation,[1] experience,[2] phenomena, functions and relations,[3] act and content,[4] and the percept.[5] Recently there seems to be grave doubt even in the ranks of conventional psychology just what the existential is. We may have existential processes as well as percepts and the latter are meanings. That is, even the psychology of perçeption is a technology.[6] Attributive dimensions are now the existential.[7] These are treated as objects in practice and as abstractions in theory. Indeed we never know whether a given is to be treated as a given, or whether it is to be treated as an abstraction or an interpretation.

It is evident that the notion of given does not include its criterion or any assistance in recognizing it. Even if we agree that it is a mental 'process,' there seems to be no way of

[1] Mach, 'Analysis of Sensations.'

[2] Avenarius, '*Kritik der reinen Erfahrung.*'

[3] Stumpf, '*Erscheinungen und psychische Funktionen.*' Cf. Rand, PSYCHOL. MONOG., 1913, No. 67.

[4] Brentano, '*Psychologie von empirischen Standpunkt,*' 1874.

[5] Titchener, 'Beginner's Psychology,' 1916, p. 112, 114.

[6] Titchener, *Amer. J. Psychol.*, 1924, p. 156.

[7] Boring, 'Attribute versus Sensation,' *Amer. J. Psychol.*, 35, 301–304.

ascertaining what is and what is not the existential, for there has been almost no agreement as to what constitutes process and what constitutes evaluation. We have had awarenesses of 'butness' and 'ifness' as givens.[1] It has been insisted that they are evaluations of the given. 'Intentions' and thoughts have been reported as existential, but these have likewise been branded as reflections.[2] Consciousness of self has been reported as a given only to meet with the same criticism from the psychology of datum. All of these alleged givens are statements of meaning and not descriptions of content. They are attempts to explain, confused by faulty analysis with immediate experience, due to a 'meaning-error.'

What, then, is the criterion of evaluation as opposed to description? Obviously, 'that which stands up under observation' will not differentiate the given from the interpreted. Even the process-psychologist himself is able to observe to his satisfaction what is evaluation and what is not. It comes to him as fact. If one wishes to distinguish between what is given and what is not, he must use some other criterion than the alleged difference between process and meaning for both come under the classification of the existential.[3]

Evidently the notion of existentiality has snagged upon the old problem of structure and function. The advocates of process-psychology have attempted to define the given in terms of content, relegating meaning to the category of evaluation, attempting to avoid the functional problem by having recourse to the expedient of 'process.' Accordingly, the real functional problem has always caused them trouble and they have taken the obvious way out by declaring that there is no valid functional psychology.[4] That is, the best we can do is to render statements of meaning which are personal, and resort to a technological procedure!

[1] James, Wm., 'Principles of Psychology,' 1890.

[2] Cf. Titchener on Marbe, Messer, Watt, and Bühler, in 'Experimental Psychology of the Thought Processes.' Lecture IV.

[3] Whether quantitative results and the concept of stimulus-error furnish a criterion will be discussed in a subsequent paper.

[4] Titchener, *Amer. J. Psychol.*, **32**, p. 519–542.

XUM

It seems obvious that what is given in one situation is interpretation in another. If the naïve introspecter 'feels tired' and knows it, that tired feeling is a given. If he reports "I wanted to go down town," his wanting to go down town was a given. Although it was in actuality a complex process, it seemed perfectly simple and immediate to him, and it stood up under observation.

In face of all these considerations how can we say that the process of knowing is a constant and therefore negligible? But let us go on. Our naïve introspecter reports: "I wanted to go down town because I had an appointment with a friend." Now, with respect to the want, he is interpreting. But he knew the reason. It came to him as fact. It stood up under observation. It was a meaning which he could analyze in terms of meaning. What is given therefore depends entirely upon what one is after. If he is seeking sensations and images, wants are not given with respect to these sensations and images. If one is after thoughts and wants, why he thought and why he wanted are reflections. If one is after causes, they are, as situations, given.

Where are we to stop? We venture the opinion that there is not a reflection, a value, a cause, a relation or an influence which is not a 'given' for somebody as a situation which stands up under observation and comes to the observer as fact. Moreover, it is content. A business concern is a given to the financier; 'party' is a given to the politician; life is a given to the physician; virtue is a given to the moralist; cause is a given to the logician. Why not? Is there a metaphysical criterion of the existential?

We have thus reduced our problem to this: In what circumstances and to whom is a situation given, not that it is or is not given. *The criterion is one of use.* When we urge the introspecter to confine himself to the existential, we should not set up for him any arbitrary criterion and then systematize upon the basis of his reports. We are simply making certain practical assumptions not to be carried beyond the methodological sphere. If what one observes is not what the other observes, the trouble lies in the different degrees

to which analyses are being made and to which the observer is synthesizing. We can do no better than to agree upon a starting point for description. The whole matter is empirical and therefore useless by itself as a systematic concept. We have learned from experience to recognize certain standards of scientific endeavor and that when these standards are violated errors or disagreements follow. We have likewise learned to know under what conditions we see, hear, feel, et cetera, in agreement with others and to some extent we are able to agree when our experiences are relatively simple and when they are relatively complex. But we have advanced no further than this.

We are still in trouble, however. The child fails to differentiate between 'real' objects and objects of imagination; and there are plenty of insane whose judgments of fact can hardly be relied upon. Are devils and snakes existential for the victim of *delerium tremens?* Is it not as dangerous to force existentiality upon the datum as it is to force Gauss upon nature?[1] The only criterion of the given, we insist, is an empirical one.

Finally, if we inspect the statement carefully that the psychological datum is that which stands up under observation, we find that after all we are not stating the truth, for to analyze a complex means to destroy it, and whenever we attempt to pass judgment upon a content of consciousness we are adding content to it.[2] We are committing the meaning-error. In any case it gives way to something else and this fact is discernible by introspection. The datum is merely the starting point of a series of interpretations, and where the given ends and the interpretation begins is indeterminable. The best we can do is to accept a practical criterion.

As a consequence of all this we seek the aid of logic in defining the datum and have recourse to the concept of function as well as content. We may satisfy ourselves that the datum is experience, but we inferentially go behind the experience in giving heart to that satisfaction. We posit a

[1] Boring, *Amer. J. Psych.*, 31, 1–33.

[2] This point will be discussed in detail in a forthcoming article.

XUM

real on the basis of agreement, objectify that real and make it independent of experience. We give to it a dynamic and qualitative as well as a static and quantitative connotation. The datum becomes an interpretive as well as a descriptive thing; i.e. it contains an interpretive as well as a descriptive element. Merely as content, or structure, or as existential it is unintelligible and uncommunicable. In practice we always make it 'content-in-use' and investigate it as such. The purest description is a technology.

Data, defined as givens in the conventional sense, are nothing more than statistical tools good for purposes of prediction on statistical grounds because they happen to work. There is nothing about them which forces upon the observer the judgment that they are facts. That they are facts is an interpretation of the observer and this interpretation depends upon relationships between data as much as it depends upon the data themselves. Thus, the notion of existentiality means a separation of interpretation from the content interpreted, denying the legitimacy of a functional viewpoint toward the given. This violates the structure-function logic and leads to an unsound psychology. The error of this position is observable on all sides. For instance, it implies the passivity of the observer and a 'coming' of the datum, both of which are falsifications of fact. Further, it leads to such an artificial distinction as that between process and meaning with its correlate, the stimulus-error.

It is evident that there is nothing particularly true about the dogma that science is pure description. We may confine an observer within certain limits to a description of relatively simple processes and by the proper instructions we can prevent him from elaborating too much upon his percepts. But in psychology as elsewhere empiricism knows no criterion of truth except agreement and that convenient measure, the probable error.

Finally, the false character of the theory of 'givens' reveals itself in the systematic use of sensation and attribute as abstractions from the given with the resulting implication that the products of analysis are not perceived. Abstractions

other than percepts are something of which we know nothing and, by definition, something of which we shall never know anything. What advantage have they over the subconscious? If the psychological unit or descriptive element is a mere logical construct, a point of reference, it would be better to dispense with it. On the other hand, if psychology is primarily a study of percepts it must deal with percepts from beginning to end. Moreover, if an element is not observable now, in due time and with good luck it should be, else the psychologist has good reason for assuming that he is on the wrong track.

IV

In trying to overcome the difficulties of separating subject from object by having recourse to the theory of existentiality our first mistake was apparently the belief that, in order to avoid an epistemological question, we must dispense with the problem of knowing. On the contrary, our difficulty lay in the neglect of a genuine psychological problem. We were misled by assuming that the problem of knowing necessarily involved us in metaphysical ultimates. We are not, as psychologists, seeking absolute truth or a criterion of knowledge, but we are confronted in any experiment involving introspection with the problem of interpretation. Psychology has before it the task of solving the problem of knowing, descriptively, before it will become a genuine science.

Our second mistake lay in the assumption that two terms —cause and effect, subject and object, content and interpretation or whatever one wishes to call them—were, for all practical purposes, furnished in the notion of the given. Attempting to combine two logically distinct meanings into one is equivalent to their separation by an unknown. In either case it is impossible to reason from one of these terms to the other.

In defining the psychological datum, therefore, we cannot legitimately assume a conscious and an unconscious, nor can we make it a unity without parts. Forced to describe function in structural terms we can do no better than to

accept whole-heartedly the principle of relativity. The datum is a relative matter, undefinable in terms of an absolute, the given. Conscious units are complexes, patterns whose limits of complexity are determined by the instruments with which they are observed. Last terms are patterns. The datum is a derived pattern. As long as the processes of derivation are uniform there is a possibility of a science. If the attitude is unanalytical, the datum is a relatively complex synthesis; if the attitude is analytical, it is a relatively simple synthesis. The difference between process and meaning represents not a classification of data but an unverifiable hypothesis.

Theoretically, this pattern or complex, in its simplest form, is an activity describable by a reference to two states or contents. These contents are percepts to be regarded as points of reference only with respect to the complex of which they are parts, not points of reference as systematic terms. In the latter rôle they are genuine percepts having both structural and functional properties.

The logical difficulty lurking behind the concepts of subject and object seems to have been overcome. The latter are the two dynamic parts within an organic unit. The act of knowing becomes descriptively a dual process of elaboration and analysis (extension and intension [1]), the simplest form of which is a pattern whose parts may be said to derive their existence from each other.[2] For example, the simplest pain consciousness is not pure pain but a pain-plus and the other increment is kinesthesis, visual imagery or some other content which is mistakenly called a surrogate. A 'conscious'-pain is merely a more complex—an elaborated—pain consciousness. In a last analysis 'pain' does not refer to a single content or quality but to the complex as a whole. It is meaning. So with all qualities and attributes. Throughout the 'stream' of consciousness the co-existence of members within a pattern is maintained in virtue of a continuation in the pattern arrangement.

[1] Cf. Titchener, *Amer. J. Psychol.*, **23**, 178.
[2] Cf. Wheeler, R. H., *Amer. J. Psychol.*, **34**, 194 *f.*

We are still adhering to the conventional context-theory of meaning,[1] supplementing the structural view of it, however, with a functional one. Functionally, meaning is motor adjustment. On the basis of these combined views one feels pain only when the stimulus arouses a pattern consciousness. A preliminary or initial contraction supplies kinesthesis,[2] the second mode of activity required by the synthesis. It is possible that in synesthesia, visual imagery may become a substitute for the kinesthesis owing to a conditioning process.[3] In other words, the arousal of a mental process depends upon a primary and a secondary stimulation. A curious demonstration of this situation is apparently found in cases of marked synesthesis in which the primary stimulation is so overshadowed by the secondary that the resulting consciousness partakes more of the second than of the first increment.[4] In the relatively asynesthetic individual the character of any specified experience such as a tone-consciousness is at least a dual process. Aside from the auditory increment there is a visual, a kinesthetic or some other 'secondary sensation.' In any event the datum is a complex and is developmental.

To recapitulate: We have attempted to show, first, that the datum of introspection is not to be defined adequately by having recourse to two successive and unlike processes—the conscious and the subconscious; that second, it is not to be defined systematically as a given. We have tried to throw light upon the situation by showing that these two points of reference constituting the subject-object relation are increments of a pattern or organic unit where we have at least a dual process and at the same time functional unity. This would mean that no analysis or description of any mental process, no matter how simple, is trustworthy unless the complete introspective method is employed; and it

[1] Titchener, 'Text-Book of Psychology,' 1911, pp. 367 *ff.*

[2] Herrick, 'Introduction to Neurology,' 1918, pp. 342 *f.* Dewey, John, 'The Reflex Arc Concept,' PSYCHOL. REV., 3, p. 357. *J. Phil.* etc., 9, pp. 664–668.

[3] Wheeler and Cutsforth, 'The Synesthesia of a Blind Subject with Comparative Data from an Asynesthetic Blind Subject,' *Univ. Ore. Pubs.*, 1, No. 10, pp. 94 *f.*

[4] We use the word, increment, because the percept is developmental.

would mean, also, that any judgment passed upon a supposedly simple and isolated content commits the meaning-error.[1]

Further, we have attempted to show the absurdity of ruling dynamism out of psychology,[2] thus reducing it to a mere statistical procedure. In this connection, we have pointed out that the datum is no more content than it is function. Accordingly, the distinction between process and meaning is fictitious. Our only way out is to accept the concept of the dynamic and treat mental processes in use. The application of statistics and other mathematical procedures will then help us as much as it would in any other circumstances, and more, because it would be more intelligible.[3] Squeeze dynamic connotations from our concept of the datum and there is no science of psychology. The goal may be statistical uniformity and a probable error of zero, but this is a goal which is logically incompatible with the alleged ideal of a static universe!

All this means, of course, that psychology should not tie itself to its own unique concepts and methods any more than the other sciences do. Its data are certain differentiæ abstracted from 'the world of experience,' but these data are meaningless when defined in terms of themselves. Accordingly, it would seem to be a distinct step in advance that psychology is slowly dispensing with those arbitrary notions which have divorced its field sharply from others. When it ties itself to broader concepts in common with other descriptive sciences and makes consciousness a genuine part of nature with no parallelistic, dualistic, and epistemological qualifications, progress will take place by leaps and bounds.

[1] The next paper will be devoted to these latter problems.

[2] Cf. Bentley, Madison, *et al.*, PSYCHOL. MONOG., **30**, 1–15.

[3] Bentley has recently taken a point of view with which this notion harmonizes. See 'The Field of Psychology,' 1924. However, he confines his functional viewpoint to a consideration of the organism as a physical instrument. For him, mental processes are not dynamic. This arbitrary separation of mind and body implies a dualistic position. In our own view there is no more objection to the interpretation of mental phenomena in terms of the dynamic than there is of envisaging the flow of water in a stream-bed as dynamic. The energy is derived from relationships between the water and things external to it, and from its constituents. Likewise, with mental phenomena if we are only permitted to accept a monistic view.

Lastly, we have attempted to point out, in passing, that 'pure description' is a technological procedure *par excellence*. The distinction between pure and applied science is based on sentiment, not reason. The investigator who assumes the attitude of the 'pure scientist' is looking upon his work just as the artist looks upon his. 'Pure science' is a technology in the persuance of which the scientist seeks self-expression. Other differentiæ of pure and applied science are economic and social. And finally, there is the important distinction that 'pure science' is experimental in a literal meaning of the word, while applied science may be merely the process of using the results of experiment. Hence, in the first case the motive is a passion for creative work, while in the second it may be financial only. We characterize 'pure science' as a technology because inevitably it is a study of things-in-use whether that use be immediately applicable to industry or not. That is, 'use' does not differentiate the pure from the applied datum.

XUM

MECHANISM AND TELEOLOGY IN PSYCHOLOGY

BY HOWARD C. WARREN

Princeton University

One of the tasks of systematic psychology is to bring psychology into line with the other sciences. How do its phenomena fit into the general scheme of natural events? How far are its laws special phases of universal laws?

At present two radically opposed conceptions are held by psychologists. The mechanistic interpretation of nature seeks to explain mental occurrences in terms of transformations which proceed according to physical and chemical principles. The opposite type of world-theory takes a number of different forms, such as vitalism, interactionism, purposive psychology, and self-psychology. Theories of this type agree in holding that a new and distinctive sort of causal process occurs in connection with the activity of 'consciousness.' According to this conception, an intelligent being acts as he does in certain cases 'because he is conscious,' or because he realizes the outcome of this and that line of behavior. It is assumed that consciousness is able to modify the ordinary course of events; and this implies that the physicochemical system of relations and transformations may be altered by something outside that system. For convenience we shall call this type of view the *teleological* conception, since its adherents generally emphasize the purposiveness of the events so determined.

However vehemently the mechanist may challenge this interpretation of nature, he is bound in fairness not to prejudice the inquiry by insisting that such a view involves a violation of physicochemical principles or a contradiction of the established laws of nature. Nor should he dogmatically reject at the outset the hypothesis of a non-mechanistic agency. The question calls for empirical adjudication.

And the teleologist is bound by scientific canons to eschew

a similar dogmatism. Because the actions of intelligent creatures appear to be guided or directed or determined by consciousness, it does not follow that the operations involved may not ultimately be analyzed into combinations of elementary factors which taken separately are physical and chemical in character. Granted that the synthetic operation—purposive behavior—appears radically different from ordinary physico-chemical processes, in reality it may be merely a higher complication of these, just as calculus is a higher complication of elementary mathematical processes.

To discover the truth the mechanist and the teleologist must join in testing their conflicting hypotheses according to the recognized canons of scientific method. This paper proposes to discuss the conditions of such a procedure.

First of all, we should endeavor to avoid prejudicing the conclusion by the implications of our terminology. The term *consciousness* is most likely to cause trouble. The teleologists, by their hypothesis itself, assume consciousness to be an active or directive force. Since this is the very point at issue, our tentative definition of consciousness must exclude any such implication. On the other hand, the radical behaviorists reject the study of consciousness altogether, on the ground that it is not a scientific datum. If we adopt this assumption we are completely debarred from making the proposed examination.

The leading definition of consciousness given in Baldwin's dictionary calls it, "The distinct ve characteristic of whatever may be called mental life." The first of several alternative definitions agreed upon by the Terminology Committee of the American Psychological Association, designates consciousness as "The distinctive basal characteristic of mental life in actual process." [1] Definitions of this type do not help us, for they offer no hint as to the *nature* of this distinctive basal characteristic. The Century Dictionary defines consciousness as, "The state of being the subject of personal and unshared experience." Obviously this conception would not suit the behaviorists.

[1] PSYCHOL. BULL., 1918, 15, p. 90.

XUM

The writer has been accustomed to regard *consciousness* as an abstract term corresponding to a concrete fact: *experience*. "When a living being is receiving sensations and having experiences he is conscious." [1] It may reasonably be objected that this conception is of no value and is perhaps lacking in meaning unless one holds the double-aspect view of mental phenomena. But with some slight modification this definition (or conception) seems to provide a proper starting-point for our study.

Let us define *consciousness* provisionally as, "The *reception and integration by an organism of the effects of stimulation, plus any significant or distinctive feature of such reception and integration* that may be discovered as a result of investigation." This definition seems broad enough to cover all standpoints. The radical behaviorist can accept it with the comment that there is nothing especially unique about reception. The extreme teleologist may be willing to adopt it in order to bring out the unique character of consciousness as potency. Those who hold intermediate views should find no difficulty in using it provisionally.[2]

Objections may be raised to this definition on the ground of its vagueness. Is it not, like the planks of a typical party platform, so equivocally worded that those holding the most diverse opinions can persuade themselves that it expresses their viewpoint? In a word, Does it not obscure the issue?

I believe this objection to be without justification. We are not trying to veil important differences of opinion under a vague formula. We merely adopt a partial and flexible formulation as preliminary to an investigation which may eventually determine the actual value of consciousness—just as the mathematician starts with an undetermined X whose value it is his task to discover. Our definition of consciousness makes it equivalent to $A + X$, where A is the reception and integration of impressions through the medium of the neural mechanism, and X remains to be determined. In the end, (1)

[1] 'Elements of Human Psychology,' Glossary, p. 399.

[2] It may be urged that this definition would attribute consciousness to the reflexes. So it would, (1) if reflexes possess the same distinctive features as do the higher forms of responsive activity, or (2) if neither higher nor lower responses possess the distinctive characteristics assumed by the teleologists.

it may be found that $X = 0$ (the behaviorist's assumption); or (2) X may prove to be incommensurable with A (the teleologist's assumption); or (3) X may be shown to have a value which is comparable with A.

Passing now from definitions to facts. The phenomena we are concerned with are in some way bound up with the stimulus-response sequence of events. Consciousness is preceded by stimulation and by neural transmission of the effects of stimulation. No one today questions the importance of the receptors and sensory nerves in the operation of receiving impressions. If the eye, the ear, the system of touch corpuscles, or some other receptor is injured or destroyed, the corresponding stimulus has a different effect on the creature or has no effect upon him at all. If the optic nerve, the auditory nerve, or some other sensory nerve is severed, the appropriate stimulus is likewise without effect. We are able at present to trace the effect of stimulation step by step from the original source to the central region of the nervous system. The problem remains, What takes place *there*?

Suppose a man sitting with open eyes facing an electric bulb. A light is switched on and he reports: "I see a light." This report takes place *after* the reception of the neural effect in the brain centers—it is a later event in the temporal succession of occurrences. We may assume that the same physiological operations take place in the visual receptors and throughout the course of the optic nerve whether the individual afterwards says, "I see," or makes no such report. If we study the phenomenon from the behavior standpoint, then, the actual form of response is determined not solely by the nature of the stimulus, nor by the condition of the receptor and afferent conduction paths, but in a measure by *central conditions*—that is, by the condition of the brain centers and their interconnections at the moment when the incoming impulse is received. In other words, the nature of the response is differentially determined by the conditions under which the separate sensory impulses are integrated into a central neurophysiological 'pattern.'

How does this description compare with the course of

events as conceived by the introspective school? The optic nerve is a bundle of fibers, and these fibers originate at various points of the retina. Each single excitation is a message from some specific part of the visual field; the total excitation reports to the brain the situation present in all parts of the field at the given moment. It is needful, then, to amend the interpretation of perception which realistic philosophers have imposed upon introspective psychology. My revered teacher, James McCosh, insisted that in the act of perception the mind 'looks out' at the world of things and perceives objects and their relations as they actually are. A similar idea appears to underlie the interpretation of perception adopted by certain contemporary introspectionists. They emphasize the distinction between '*object* of consciousness' and '*content* of consciousness.' Just what does this distinction mean? Granting the fact of 'introspection,' what the observer observes directly in the instance chosen is not the electric bulb nor the stimulated retina, but some central occurrence—some physiological process in the central nervous system. If he sees a surface or a contour of light, he is observing or experiencing a whole group of physiological processes at once.

The expression 'object of consciousness' is apparently ambiguous: It may mean (*a*) the original *source* of stimulation—in this case the electric bulb; or (*b*) the impinging *stimulus*—i.e. the image or picture of the bulb on the retina; or (*c*) the complex impression made *at the nerve center*—here, the visual center in the cortex. The philosopher, whose interest is epistemological, naturally emphasizes the first of these meanings. But our analysis indicates that this would not fit the psychologist's problem: A misty atmosphere between the bulb and the eye alters the effect of the light on the observer, but it does not alter the glow of the bulb. Nor is the second meaning satisfactory: The retina is composed of separate rods and cones with separate nerve connections leading to the visual center; there is no general integration in the receptors. For psychological purposes, then, the 'object of consciousness' is not the original generator of the group of stimuli, nor the physiological condition of the receptor brought about by the

stimulation, but the effect of the external situation upon the brain of the individual. The original data may have been greatly altered by various intervening forces and influences. And, since the central effect of any stimulus is determined in great measure by the momentary condition of the central mechanism, we must extend the list of intervening influences to include those that affect the brain itself. In other words, if we adopt the introspectionist's viewpoint and recognize the existence of 'objects of consciousness,' these objects are actually brain states or neurophysiological patterns.[1] The 'content of consciousness' (if we use the term) is the experience induced by the object of consciousness. Thus from the introspectionist's standpoint, just as from the behaviorist's, we are led to the central nervous system as the focus of the receptive process.

And now the question arises: Has the distinction between object and content any real significance? We are presumably dealing with actual events and processes or situations. The term 'object of consciousness' designates a certain definite phenomenon, the term 'content of consciousness' also designates a certain definite phenomenon. Do these terms denote two different phenomena, or are they merely two terms applied to the same phenomenon?

Historically several different hypotheses of the relation have been proposed: (1) *Epiphenomenalism.* The object of consciousness determines or influences the content of consciousness, but not conversely. (2) *Interactionism.* The object of consciousness influences the content and conversely the content has a direct effect upon the object. (3) *Psychophysical Parallelism.* There is no causal relation whatever between object and content; the two series of events run along synchronously. (4) *The Double-Aspect interpretation.* Object and content are identical; there is actually but one series of events, which manifests itself in two ways. When one examines the structure of the brain and the physiological processes therein, he is observing these data indirectly, through

[1] If we regard psychology as the study of the entire reactive process, we may properly use the term *object* (not object of consciousness) to designate the ultimate source of stimulation.

visual and other sensory stimulation; the original occurrences have been modified and transformed, somewhat as human vocalization is transformed from sound-wave vibrations to electric vibrations in the telephone. When we observe our own experiences we are *living the brain events* directly; they are not transformed, but actually experienced. (5) *The Behavioristic interpretation.*[1] The behaviorists, in accordance with their objective method, banish the 'content' of consciousness from science altogether. They are concerned with objective phenomena only. For them the distinction between object and content is either meaningless or scientifically valueless.[2]

In attempting to define the issue we have run athwart a metaphysical problem. Scientists are constitutionally averse to metaphysical discussion on account of its traditional association with verbal and logical jugglery. But this particular problem cannot be wholly avoided in any thoroughgoing investigation of organic phenomena. The classification just made seems to disclose the focal point of empirical inquiry: What takes place in the brain when a group of sensory impulses arrives there? What is the nature of the integrative or adjustive process?

If convincing evidence is found that the course of physiological activity is altered by mental activity, then we are obviously led to Interactionism. If overwhelming evidence is found to the contrary, Interactionism goes by the board. Interaction is a scientific form of dualism; it lies at the basis of the teleological interpretation. The Double-Aspect view and Behaviorism are two distinct forms of monism; they agree in adopting a mechanistic interpretation of organic responses. In the light of present knowledge these three theories seem the most reasonable alternatives.[3]

The decision between these conflicting views will depend

[1] Historically this has been called *Materialism.* The term has acquired an emotional connotation which makes its use undesirable in an unprejudiced survey.

[2] An additional (sixth) hypothesis is *Mentalism* or Spiritualism. It would carry us too far from scientific objectives to discuss this view here.

[3] Epiphenomalism and Parallelism are both dualistic, but in each case the duality appears to be redundant.

upon the discovery of certain facts as yet unknown. It hinges upon the outcome of crucial observations and experiments. Elsewhere [1] I have indicated a line of research which promises to throw light on the issue between Behaviorism and the Double-Aspect view. Our present interest is the issue between the Teleological and Mechanistic conceptions of the universe. Here too it seems to be a question of facts. The problem is to arrange crucial situations and to devise apparatus for observing and recording the outcome. But before this is attempted by laboratory workers it is essential to know where the crux lies. In order to discover this let us examine the stimulus-response phenomena.

In our example of the electric light and the individual observing it, we noted that the light stimulates the retina and produces activity in the optic nerve, which activity is transmitted to certain regions in the cortex known as the visual centers.[2] Thereupon the separate nerve impulses from the various optic fibers are integrated into a 'pattern.' Following this integration a 'response' takes place sooner or later. This response, the outcome of stimulation, depends upon a number of factors. Assuming the uniformity of nature, any difference in response on two occasions will be due either to a difference in the stimulus, or to a difference in internal conditions (including consciousness), or to both.

We may classify the internal factors which affect the response into the following groups: (1) Transitory variations in the condition of the receptors, transmitting neurons, and effectors. (2) Transitory variations in the condition of the various synapses and higher nerve centers. (3) More permanent structural alterations in the receptors, conductors, and effectors. (4) More permanent structural alterations at the synapses and centers. (5) Active intervention of consciousness, modifying the course of neural processes, if the X of consciousness prove to be an effective agency.

In addition two factors should be noted which are not internal: (6) The response may take a different form owing to

[1] 'Psychology and the Central Nervous System,' PSYCHOL. REV., 1921, 28, 249–259.

[2] The exact nature of the nerve impulse is not especially germane to our present problem.

differences in the environment on which the activity is exerted. Here the initial phase of the response is the same, but its progress is facilitated or impeded or modified by the surroundings on which it operates.[1] (7) The response may be modified by the effect on the brain of simultaneous stimuli from other senses. This of course means that the *total stimulation* is different in the two cases. It has no bearing on the process of reception, but it is an important factor in the integrative process.

Factors 1–4 may be called *neurological*, factor 5 *psychical*, factors 6–7 *external*.

For present purposes the transitory variations (factors 1 and 2) can be grouped together. They exert an influence which is traditionally known as *fatigue* when the response is impaired, and *restoration* when the fatigue effect has disappeared. The more durable alterations (factors 3 and 4) are traditionally called *practice*, *exercise*, or *habit*.

We have thus two sorts of neurological factors which alter the response: the temporary and the durable; and these are opposite in character. (1) Given the same stimulus repeated again and again, there are certain intraorganic factors which render its effect progressively less powerful; the efficiency of the stimulus tends to be *impaired* by repetition. (2) There are other intraorganic factors which assist the course of neural activity, and this tends to *improve* the response when the same stimulus is repeated.

Obviously we are dealing with two entirely distinct sorts of factors, since they work in opposite directions. It seems probable that one of these types of operation is chemical and the other physical. Physiological research has shown that the use of an organ entails catabolic changes in the tissues—fatigue, exhaustion,—while a period of rest serves to restore the organ—to build up new material anabolically. These temporary changes, whether in the receptor or in the neural substance, are apparently chemical in nature. The durable

[1] E.g. (*a*) Facilitation: Raising an aluminium pitcher when we are adjusted for a heavier metal. (*b*) Impediment: Attempting to squeeze a solid rubber ball when adjusted for a hollow one. (*c*) Modification: Stepping up one more stair than is actually there.

changes, on the other hand, may be regarded as mainly physical—as due to changes in the space relations, configurations, etc. of the separate neurons and their connections. Whether this distinction between chemical and physical factors is true, is a question of fact to be settled empirically. There is no doubt about the existence of two opposite influences.

Teleologists generally admit the physiological nature of the fatigue effect. The phenomenon of sleep, the effect of drugs, bodily ailments, etc., on mental activity, and the exhaustion which follows prolonged labor, appear to establish the fact that the temporary impairment is due to neural factors and not to the intervention of consciousness.

The mechanistic character of the durable changes is not so clear. The formation of a habit usually means the acquisition of a more efficient, a more *useful* type of response. According to the mechanist the nerve impulse always follows the path of least resistance. How can the course of the nerve impulse be diverted from the usual channels unless some intelligent agent, or agency, intervenes so as to alter this path of least resistance and shunt the nerve current?

The acquisition of new responses is an unquestionable fact. It appears in even the most elementary type of reaction. The knee-jerk is one of the most automatic of responses. And yet, by dint of repetition this response may come to be aroused by the mere sight of a hammer moving towards the knee, even when there is no actual impact. Now if we consider the sight of the falling hammer as the stimulus to be investigated, we find that at first it does *not* bring about the knee-jerk response. Later, when combined with the impact stimulus, it does yield this response. Finally, even without the impact the new response occurs.[1]

This is one way in which durable changes in response are mechanistically brought about. There are other types of modification as well. *Inhibition* of the usual response may cause the reaction to assume a new form, which may come to

[1] This should be substantiated by experimental evidence—comparing the voluntary attempt to produce the movement before training, with the movement produced in the conditioned reflex.

XUM

be permanent. Or an *unusually powerful stimulus* may bring about a more complex response, opening up new pathways that ordinarily are less permeable; thereafter these pathways may share in the response.[1] These and other possible sources of durable modification of responses, or habit formation, require thorough investigation. We can at least assume (1) that certain changes in the form of responses are brought about through complications of stimuli, (2) that the new forms of response so acquired may become permanent, and (3) that the means by which the changes are effected are in many cases purely mechanistic—that is, independent of any directive consciousness in the teleological sense. These changes are due to complex stimuli and to ensuing complications in the neural effect (factor 7). They are phenomena resulting from the integration of nerve impulses.

The teleologist admits all this so far as reflexes and other automatic actions are concerned. He bases his case upon certain specialized types of responsive activity which involve the higher brain centers. The responses which he considers non-mechanistic are what are generally called *intelligent* acts. Intelligence is said to involve (1) *conscious anticipation*—that is, prevision or forethought; and (2) *conscious selection*—that is, realization that a certain one among several anticipated acts is more suitable or useful than the others, this realization leading to the performance of this particular act. Let us examine these two characteristics.

Elsewhere [2] I have discussed the nature of anticipatory action. It appears that even without 'thought' in the traditional sense of that term, a creature can anticipate a coming situation and begin an act which is in effect a response to a future stimulus. This is due to the possession of *distant receptors*, which furnish advance stimuli that herald the rest of the group of stimuli. The creature sees the object approaching before it comes in contact with his body and he thereupon starts to respond. The *brain centers*, both lower and higher, enable him to integrate the entire group of im-

[1] For discussion of the alternative means of producing changes in motor expression see 'Human Psychology,' pp. 97–99.

[2] 'A Study of Purpose,' *J. Phil.*, 1916, 13, 5–26, 29–72.

pressions and initiate the first stages of the response. So far as unreflective anticipatory or purposive actions are concerned, then, the mechanistic explanation appears sufficient to account for their existence. Purposive actions of this automatic type are operated by a neural structure which has been built up phylogenetically by Natural Selection. And, since the thinking function is dependent upon cerebral activity, and the evolution of the cortical structure is apparently due to Natural Selection, it seems not improbable that the origin of conscious forethought may be explained in the same mechanistic way.

The problem focuses, then, on the second characteristic,—*conscious selection* of that particular form of response which appears to be the most suitable of several alternatives. The contention of the teleological psychologists seems to rest upon the efficiency of *intelligent awareness*. In intelligent activity we think of several alternative courses of action, and because we are intelligent beings we realize that one is preferable to the others. The phenomenon of thinking, or considering the various alternatives, is called *deliberation*. The final decision is called *choice*. Our problem is to discover whether deliberation and choice are mechanistic processes, or whether the additional *X* of consciousness contributes some essential element to the chain of events and the outcome.

How shall this problem be experimentally investigated? If we could observe the elementary physiological processes in the brain, an answer might be readily obtained. But this method seems scarcely available in the near future. The behaviorist treats thought as a succession of implicit motor phenomena and seeks to evaluate the central brain processes by means of these manifestations. If this view is correct, we may investigate the problem by behavioristic methods. But there seems reason to doubt whether the motor phenomena in question are essential features of thought.[1] There remains the introspective method, and however questionable its validity may appear in the eyes of the behaviorist, it is the method most acceptable to the teleologist. If its data discredit his view, the teleologist has no recourse.

[1] The behaviorists' conclusion appears to involve the 'thermometer fallacy.' See 'Psychology and the Central Nervous System,' PSYCHOL. REV., 1921, 28, pp. 253–255.

To reach a solution would require a long series of graded observations. Let us take some typical cases varying in complexity, to show how the introspective method may be used in attacking the problem.

1. The observer is confronted with a form-board and asked to fit the accompanying blocks into the depressions. Being an intelligent person he decides at once where each block belongs, and accomplishes the task immediately without deliberation. On bare inspection of the various forms the adult subject is aware of the correspondence. But is this awareness a pure act of consciousness? A child finds difficulty in selecting the proper blocks. A blindfolded adult experiences similar difficulty. Both proceed by trial and error to establish the correspondence. Once they have thoroughly learned the form-relation, the selection comes quickly in the future. In this case intelligent selection is not the outcome of bare inspection; it is the result of learning.

2. I am asked to add up a long column of figures. Bare inspection does not give the sum. I start at the bottom and add successively till I reach the top. Then I proceed to verify the result by starting at the top and running down. If the two results agree I am convinced that my answer is correct. Sometimes they do not agree, and I repeat the operation. What does this show? The fact that I sometimes make an error casts doubt upon the efficiency of intelligence. I say (or think) that $9 + 7 = 16$ because of previous training, rather than because I am 'intuitively' conscious of the relation. Such judgments are due to the recollection of acquired central associations. The specific number-associations involved in addition tend to become fixed owing to the uniformity of nature. We find that $9 + 7 + 8 + 4$ yields the same result as $8 + 7 + 9 + 4$ or any other permutation of these numbers, and in simple instances this result may be further verified by counting. Intelligence in this case seems to mean an ability to form certain associations and to fix or learn them and to repeat automatically the specific associations which have been learned; it does not involve 'intuition.' The awareness does not add anything to the process. The brain

process of integrating the separate nerve impulses and shunting the resultant complex impulse into some definite, acquired neural route seems sufficient to account for the outcome. The verbal response—16 in the former instance, 28 in the latter—would be the same without the intervention of consciousness. A corresponding 'response' occurs if the addition is performed on an adding machine. An error may occur on the adding machine through striking the wrong key. Our mistakes in mental adding may be ascribed to a wrong shunting of the brain current.

3. I am dressing for dinner and a pearl stud slips from my fingers and falls into a crack at the edge of the baseboard. There are several ways in which this situation may be treated—ranging from frantic activity of the trial and error sort, to calmly ignoring the lost stud and searching for a substitute. Take the case in which the situation is treated as a problem to be solved. There is no significant responsive activity for some time. Instead there is a succession of thoughts: "Must get it—no other decent-looking set—pry off board—hammer, cellar, not time enough—something near by — screw-driver — button-hook — where? — don't know — woman (i.e. a woman would have one)—hair-pin, none—knife—shoe-horn—curved, won't go in—knife—try it." This is a fairly accurate report of the thought series,[1] which ends in a movement of the hand reaching into the pocket for the knife, followed by a series of other adjusted movements, resulting finally in recapture of the stud.

In a case like this, what is the differential cause of the actual response, i.e. the hand movement? The laws of association account sufficiently for the succession of ideas; the mental pictures or representations are all familiar from repeated experience. But how is it that the ideas of *hammer*, *button-hook*, *shoe-horn*, bring about no significant motor expression, while the idea of *knife* does? For one thing there is no inhibitory association with the knife as there is with each of the others. But still, the idea *knife* at its first occurrence

[1] Many items in the series are abbreviated or syncopated. There is at times apparently a symbolic representation, not unlike the use of symbols in algebraic operations.

led to the idea of *shoe-horn* and not to the hand movement. It will not do to lay too much stress on the repetition of the successful idea *knife:* for in many instances some of the inhibited ideas (button-hook, shoe-horn) may also repeat themselves in the series.

Here is the parting of the ways: (*a*) The teleologist claims that the hand response occurs because the individual *realizes the appropriateness* of this response and consciously directs the motor outgo into this channel. (*b*) According to the mechanist the thought series (speaking in neural terms) consists of a succession of physiological processes in various cortical centers, aroused one after another by a nerve current traveling from center to center [1] and passing at length over the path of least resistance into certain outgoing motor nerves.

Our analysis discovers nothing in the deliberative process that requires a non-mechanistic factor. The succession of associations can be accounted for by previous experiences, just as in the simpler cases we have examined,—each specific association has been already acquired or learned. The only doubtful point seems to be the realization that a certain alternative is best fitted as a mode of response. The teleologist's case seems to rest not upon intelligent *deliberation* but upon intelligent *choice*,—upon the fact that we consciously adopt a certain one from among the various alternatives considered during the process of deliberation.

The efficiency of choice has definite limitations. A thought has no intrinsic power to initiate a specific movement. The connection, whatever it is, between thought and innervation of muscles is acquired by experience. The child who wishes to copy a written word is unable without training to form the letters correctly. The adult who has never twitched his ears voluntarily is unable to initiate the movement at will. Many individuals are unable to draw a picture that bears any re-

[1] Actually slight motor responses take place constantly during the process; words are half formed, the brows are knit, the eyes flit from bureau to floor, etc. For the behaviorist, these motor expressions furnish the stimuli for the next cortical condition (or thought) in the series. According to the view here taken they are not significant; they do not determine the final response, but furnish merely the background or framework for the central process; the real generator of each new idea in the series is the cortical current itself.

spectable likeness to the object they are imagining or are actually observing. The directive action of consciousness, then, if it exists, is not so much a control of outgoing nerve paths as a selection of pathways in the brain centers themselves.

4. I am shown the Healy puzzle box and asked to open it. This box is about 8 inches square and 4 inches high, with a glass top which exposes the interior. I endeavor to solve the problem by observation and thought instead of by casual trial. I note that the box is fastened by a hasp, which is held fast by a long rod passing through the staple. To the left the rod ends just beyond the staple in a T; to the right it extends the length of the box, ending in a long hook in which is inserted a ring, attached to a taut cord, which passes inside the box. I observe that if this cord can be loosened sufficiently to slip the ring off the hook, the rod can be pulled out and the hasp lifted.

Within the box the cord passes at an angle through a ring which holds it taut, and ends in another ring hung on a post stationed near the front of the box. Connected with this affair is an intricate-looking system of cords and rings, two of the rings being anchored to posts. There are oval holes in the walls of the box opposite each of the three posts. A button-hook is furnished which will pass through these holes.

After examining the contrivance carefully I observe that the only ring that can be immediately pushed over its post by means of the button-hook is the one nearest the front. This will loosen the bent cord, but will not give it sufficient play to release the outer hook on account of the middle ring, which is held fast by the rest of the mechanism. But the ring anchored to the right-hand post is now sufficiently loosened to admit of pulling it over the post. This loosens another cord sufficiently to throw the ring off the third post, which releases the middle ring, giving the main cord sufficient play to pass over the outside hook. This rod can then be removed from the hasp and the box opened.

Thus I have solved the problem by observing the total conditions included in the situation before setting out upon

XUM

the actual performance. My organized knowledge of mechanical principles, acquired by past experience, recalls successively various ideas regarding the movements of rigid and flexible bodies. The deliberation, as before, follows lines that are describable in mechanistic terms. There is formed at length the idea of a certain movement which will start the proper solution of the problem.

How comes it that I select this response and actually perform it? But *do* I perform it? I may instead *tell* the experimenter what movement should be made. The selection does not lead inevitably to the performance of the selected response. Conscious choice apparently means only that a train of thought has led to a thought situation which is not inhibited or modified by ideas of difficulties. But if all our thoughts have concomitant brain states or processes, the formation of the 'solution' idea seems to be describable in terms of brain mechanics as readily as by the intervention of some teleological agency.

There remains, then, only one phase of the purposive activity that is not clearly reducible to mechanistic processes: the awareness or *realization that a given alternative will work*—that it is suitable. This 'realizing' phenomenon is Bergson's *intuition*. Is the intuitive psychosis, like the rest, explicable in mechanistic terms? This appears to constitute our final problem.

Here as elsewhere the issue calls for empirical investigation. Till this is undertaken it seems scarcely profitable to carry our theoretical analysis further. We have suggested the kind of evidence needed to solve the issue. It may be possible, by systematic research, to demonstrate either (*a*) that the entire series of events in the reactive sequence constitutes a rigid physicochemical chain, or (*b*) that at some instant during the reaction, in beings whose organism has attained a certain degree of complexity, the course of nerve current which excites the responsive activity is not fully determined by the momentary pattern of brain processes, but depends partly upon *conscious realization of meaning*. Until such crucial empirical tests are made, our adhesion to either the mechanistic or the teleological interpretation should be only provisional.

Summary.—In this paper, admittedly tinged by a mechanistic bias, the following conclusions have been reached:

(1) There is nothing unique about anticipation, prevision, forethought. The distant receptors bring advance warning of certain coming situations, which creatures with a complex neural mechanism can utilize. Neural reception and central integration apparently determine the rise of forethought, as of thought in general.

(2) Perception, or complete reception of an intricate situation, involves considerable central integration. The old notion that the mind reaches out and grasps external objects in their true relations is a reversal of the actual state of affairs. As a matter of fact the receptors get only piecemeal reports of the external situation and pass the separate data along; the integration occurs at the brain centers. This integration results in a complex pattern of physiological activity. Either this pattern *is* consciousness or it *determines* consciousness.

(3) Where perception (that is, reception plus integration) results *immediately* in some motor response, it may reasonably be assumed that the neural pattern of activity issues directly into a motor impulse, following the path of least resistance. In automatic actions our 'conscious awareness' of the external situation does not seem to add to the report received at the centers any force or influence which shunts the motor outgo into some other than the least resistant path.

(4) Where the overt response is not immediate—i.e. where the integrated reception leads to a succession of thoughts—the complete description of the causal sequence is not so evident. The mechanist rests his case on the established fact that the succession of thoughts is not unaccompanied by brain activity. (*a*) If the physiological processes are hampered by injury to the brain, or by temporary disturbing conditions as in sleep, the thought train is checked or may be diverted into other channels. (*b*) Where thinking proceeds normally, there is abundant evidence of concomitant brain processes throughout the deliberation. Our analysis has discovered no loose end in the sequence, where consciousness must be inserted to supply a missing link in the physiological chain. Neural patterns are

successively transformed into new patterns, till at length there arises a certain pattern which issues into motor activity. The initiation of responses after a train of deliberative thought thus appears to be a process similar to the initiation of responses immediately following perception.

(5) Conscious choice does not intrinsically or inevitably initiate the appropriate motor act. The connection between a decision and a specific muscular movement is acquired through associative experience.

(6) There remains the problem of 'intuition.' Does our realization or conviction that a certain alternative is suitable, add something to the total situation and modify the course of neural activity? This appears to be the crucial point on which the investigation should focus. Here, if anywhere, we should expect to discover 'directive consciousness.'

(7) The presumption at present seems to favor the mechanistic hypothesis. The burden of proof lies upon the teleologist to discover a gap in the physicochemical sequence. If no gap appears, the hypothesis of efficient consciousness seems redundant. But as suggested at the outset, science seeks the truth rather than the victory of certain hypotheses. The discovery of truth depends upon our ability to devise and carry out crucial tests. It is not attained by dogmatic insistence, by ridicule, or by invective.

PURPOSE AND COGNITION: THE DETERMINERS OF ANIMAL LEARNING[1]

BY EDWARD CHACE TOLMAN

University of California

The present paper will offer a new set of concepts for describing and interpreting the facts of animal learning. These new concepts will differ from the usual ones in not being restricted to the customary physiological notions of stimulus, neural excitation, synaptic resistance, and muscle contraction (or gland secretion). They will rather include such immediate and common sense notions as *purpose* and *cognition*. These latter, however, will be defined objectively and behavioristically, not 'mentalistically.'

More specifically, the reader is to be asked to envisage the facts of animal learning (and, indeed, of human learning as well) in terms of (I) a *goal-seeking* (purpose), (II) a set of innate or acquired *initial exploratory impulses* (initial cognitive 'hunches'), and (III) the acquisition of a set of *final object-adjustments* (final cognitions). And, in order to indicate the real fruitfulness of such concepts, they will be concretely and severally discussed for the particular case of maze-learning in rats. The necessary material for this discussion will be drawn from the published literature, from a description of experiments already performed at California, but not yet published, and from a description of experiments which ought to be performed. We begin with the matter of goal-seeking.

I. Goal-Seeking (Purpose)

When a rat runs a maze, it is to be observed that his running and searching activities *persist until food is reached.* And it appears that this persistence is the result of the physiological condition of hunger. We do not know whether the rat,

[1] Read in part before the Western Psychological Association, Stanford University, California, August 8, 1924.

in so 'persisting' is 'conscious'; we do not know whether he 'feels a purpose' (to use the terminology of the mentalists); but we do know that, given (1) the physiological condition of hunger and given (2) the objective conditions of the maze, the rat thus *persists until the food is reached.* It is this purely *objective* fact of persistence until a certain specific type of goal-object is reached that we define as a *goal-seeking.* And as thus defined, a goal-seeking is a wholly objective and a wholly behavioristic phenomenon. There is nothing 'mentalistic' about it.

The one important experiment already in the literature directly oriented toward this question is that of Dr. Simmons on 'The Relative Effectiveness of Certain Incentives in Animal Learning.'[1] In this experiment, she found that the rate of maze learning may be varied by using different types of goal-objects. She found, for example, that bread and milk, as food, caused a more rapid learning than sunflower seed. She found also that complete satisfaction of hunger immediately at the end of the run is more effective than merely partial satisfaction (followed by delayed feeding an hour later). Such findings indicate gross facts concerning the importance of goal-seeking. They indicate that the nature and strength of this goal-seeking and its satisfaction have important effects upon the learning rate. They do not indicate, however, in just what qualitative ways the different goals produce their different effects. She did not, for example, devise her experiments so as to discover whether the different effects are a matter of calling out more and better initial exploratory impulses or rather of a more rapid 'stamping in.' Nor do her results indicate whether the one type of food was better than the other because the one food was less lasting and hence the animal was more *hungry* at the beginning of each new trial, or whether it was rather a matter of the greater satisfactoriness (pleasantness) of one food as such. These are all important points left as problems for further investigation.

Under the head of such further investigations we might suggest the following. First, we might, perhaps, test the 'satisfactoriness' of the goal-object *per se.* Suppose a maze

[1] *Comp. Psychol. Monog.*, 1924, **2**, Serial No. 7.

were so constructed that the living cage of the animal were directly connected with the food box. Our experiment would then attempt to use the desire to get back home (i.e. to the living cage *per se*) as a *standard* against which to measure strengths of the food-desire. We might, perhaps, measure the time spent in the food-box as against the time spent in the living-cage in any fixed period allowed for eating, or in some similar way measure the relative strengths of the food-box-getting-to tendency as over against the home-going tendency. Having such a method of measurement, we could then discover to what extent this food-getting-to tendency *per se* varied in terms of the home-going tendency with different sorts of foods and at different stages of learning. What, for example, would be the results, given the same physiologically defined degree of hunger, for bread and milk and for sunflower seed? What those as regards the initial and the final stages of learning? Etc., etc.?

It is obvious that after gathering such data together we would have infinitely more information for explaining and describing a maze learning-curve than we have at present.

A second line of investigation might be directed toward discovering the nature of the goal features. What features of a given goal-object is it which make the latter a goal-object? Experimentally, that is, we would want, by varying the stimuli which a given food presents, to discover just what are its essential and defining goal-features. Is it, for example, its color, its taste, its smell or its what, which determines its acceptance as a goal? And does this necessary feature perhaps vary with the different stages of learning?

Finally, a third sort of investigation suggests itself. For it appears that goal-seeking must be defined not only as a tendency to *persist* in more or less random fashion *until food is reached* but also as a tendency to *select* within limits the *shorter* (and probably also the easier and pleasanter) of two or more alternative ways. This appeared first in De Camp's classical experiment.[1] For it will be remembered that he found that a rat will, within certain limits, discriminate between and prefer the *spatially* shorter of two alternative routes to food.

[1] De Camp, J. E., *Psychobiology*, 1921, **2**, 245–253.

XUM

At California we have now done a further experiment from which it has been discovered that a rat will also select, other things being equal, the *temporally* shorter of two routes. Mr. Sams, in an experiment to be soon published, found that a rat, when given the choice between two ways to food, one temporally longer and the other temporally shorter, will, within limits, select the latter. The apparatus was simple. At the exit from a starting box the animal could go either to the right or to the left. Both the right and the left alleys would lead him in symmetrical fashion and by spatially equal distances to one and the same food-box. Correspondingly placed down each of these alleys were 'confinement spaces'; each with a door in front and a door behind. When the rat reached one of these spaces he found the door in front closed and as soon as he got there the other door was closed behind him. He then had to wait between the two doors for a certain predetermined length of time, after which the door in front of him was opened and he was allowed to pass on toward food. After a few experiments the rat settled down to the necessities of the situation and took the enforced waiting calmly. The essential experimental procedure consisted in making him wait longer on one side than the other. As yet, the experiment has not been performed with a great many animals nor have the limens been determined with accuracy. But with the three animals tested it was definitely discovered that a wait of one minute would be discriminated and selected as against a wait of six minutes. And when the sides for these long and short waits were interchanged, the animal readjusted and went to the new side.

Summing up, then, from De Camp's experiment and from this experiment of Mr. Sams' we conclude that the goal-seekings (purposes) which govern a rat's maze-learning include not only the tendency toward such and such a goal-object as such but also a further selective tendency to get to this goal-object by the shortest route both (*a*) spatially and (*b*) temporally.

Finally, it appears probable that in addition to these spatial and temporal preferences, further investigations might indi-

cate that a rat will also prefer a 'physiologically easier' route when spatial and temporal distances are rendered constant.

II. Initial Exploratory Impulses (Initial Cognitive Hunches)

Given the tendency to persist *until* the food-box is reached and given the further tendency eventually to select both the spatially and temporally shorter route, what sorts of exploratory impulses are initially called into play?

The one important investigation bearing upon this point is Hamilton's.[1] Hamilton investigated with a four-box 'multiple choice' apparatus what he called 'Trial and Error,' or 'Perseverance Reactions.' He presented his animals with a series of settings in which the only essential feature was that the correct door in any given setting was never the same as it had been in the just preceding setting. When confronted with this series of practically insolvable problems, he found that the responses of his animals grouped themselves into certain very definite types:

1. "Responses which indicated an appreciation of the experimenter's rule that no alley is the right for two successive trials."
2. "Responses which indicated an appreciation that it is useless to try an alley more than once during a given trial; all four alleys tried and in an irregular order."
3. "An effort to escape by trying the alleys one by one in succession, beginning at the extreme left and working to the right or beginning at the extreme right and working to the left."
4. "More than one separate effort to escape by a given alley during the same trial but with an interruption of such efforts by an interval of effort to escape by one or more of the other alleys."
5. "During a given trial the subject enters an alley which does not afford escape, leaves it and reënters it one or more times without having tried another alley; or, having

[1] Hamilton, G, V., *J. Animal Behavior*, 1911, 1, 33-66; *Behavior Monog.*, 1916, 3, 65.

> tried a group of two or three alleys in a certain order, he reënters all alleys of the group in the same order one or more times."

Reviewing these five types of responses from our present point of view, we see that to a large extent they are nothing more nor less than descriptions of the nature of the exploratory impulses (or initial cognitions), which appeared in his different animals.

The tendency to try all doors (but each one not more than once), the tendency to work in regular fashion from left to right or from right to left, the tendency to try the same door several times, either with or without intervening attempts at other alleys, and finally, the tendency to persist at some small group of doors over and over again are pretty evidently *objective* descriptions of the innate nature, extent, and flexibility of the animals' initial exploratory tendencies (initial cognitive 'hunches' of how to get there). The animal that persists at the same door over and over again obviously has an inferior set of exploratory tendencies (cognitive 'hunches') to the one that tries other doors in between.

And it is of course clear how important information of such an order will be if we are to be prepared wholly to understand and explain maze-learning. How fast and in what manner a rat 'learns' will be conditioned among other things by the range, methodicalness, and flexibility of his exploratory impulses.

Another bit of experimental evidence bearing upon this matter of exploratory impulses is to be found in some, as yet unpublished, experiments performed at California. In these experiments a simple T maze was used. And it was arranged so that the animal could get back to the food box in identical fashion whether he chose the left or the right angle of the T. Either route met with success. The interesting point which came out and which is of prime importance to us here is that, although some animals acquired a consistent preference for one side rather than for the other, this preference often appeared relatively late and in some animals not at all and as long as such a preference was absent there was instead a very

pronounced tendency toward continuous and regular alternation—left, right, left, right, or right, left, right, left. That is, not only did the animals tend to average five times to the right and five times to the left, out of the total of ten trials on any day, but the percentages of immediate alternations from left to right and from right to left were much greater than would have resulted from mere chance. It appeared, in short, that even where either side was equally 'satisfactory' there was in our rats a positive tendency left over toward variation of response. But, if this is so, it indicates that trial and error does not result merely because of failure, but that it is a positive tendency in and of itself.

Finally, there is still a third bit of experimental evidence concerning the initial exploratory tendencies, evidence concerning immanent orientation factors which may be found to run through them. For Dashiell [1] found that the proportion of animals, who on the initial trial entered any given alley or blind, varied with the relative direction of this alley or blind. An opening straight ahead, for example, was entered much more frequently than one to the side, and on coming out of a blind, retracings were less frequent than goings-in.

So much for such experimental evidence as is now on hand. It is only a beginning. The questions concerning the causal determiners of these initial exploratory propensities is still unanswered. How much are they due to experience? How much are they conditioned by the particular type of goal and the particular type of physiological need back of that goal? Do, for example, fear and sex call out different initial exploratory tendencies from hunger and from one another, and so on? And how much, finally, do they vary in kind or extensity with the mere urgency and intensity of the need? Would we find that the more intense the motive, the more restricted the exploratory readiness, and the less intense the motive, the more leisurely and widespread the sallies? Etc., etc.?

[1] Dashiell, J. S., *Psychobiology*, 1920, 2, 181–186.

XUM

III. Final Object-Adjustments (Final Cognitions)

What are the defining elements of, and the causal factors determining, the acquisition of the final object-adjustments (cognitions)? We must note, in the first place, that such final object-adjustments seem to be made up of two phases or aspects: (1) what we may call their intent or noëtic aspects, and (2) what we may call their sensory-cue or sensory aspects. We shall consider these separately.

(1) The intent or noëtic aspect of an object-adjustment we will define as the object-structure (i.e. that behavior possibility) which the animal's behavior can be observed quite definitely to *impute* (whether correctly or incorrectly) to such and such a particular part of the maze. Thus, for example, the behavior of the rat who runs straight into the end of the shortened maze-blind may be said quite objectively to impute an object-structure (a behavior possibility) to this part of the maze which the latter does not possess. It imputes, that is, a greater length to the alley than it has. Or, again, when a rat, at a given stage of learning, tends to enter alleys pointing in the general direction of the food-box more often than those pointing in the opposite direction, such a predominance of entrances obviously states, exhibits, an imputation that the food-box lies in the general direction of such alleys. Given, in short, that the animal is dominated by the food-seeking impulse, his various behaviors in the successive parts of the maze can be said, at any stage of learning, quite objectively to express his thereunto acquired object-adjustments (cognitions) with respect to the 'getting-on-toward-food' possibilities of such maze parts. It must be emphasized that these object-adjustments to the maze structure have meaning only with reference to the task of *getting to the food and getting there as quickly, both spatially and temporally, as possible.* What the animal's behavior exhibits in the way of object-adjustments to (cognitions of) the maze structure are not, of course, comparable to what the physicist would tell us about the maze, but are merely (cognitions of) adjustments to the maze from the one point of view of getting to the food-box.

So much for an indication of the intent or noëtic aspect of the animal's object-adjustments.

(2) It appears, however, that learning consists not merely in the acquisition and improvement of these intent or noëtic aspects of the animal's adjustment to the maze, but it involves also the attachment of such intents to the *right stimulus-cues*. The functioning of any given object-adjustment requires the presence of stimuli. The rat imputes a behavior-possibility to a particular part of the maze as a result of particular stimuli presented by that maze part. And the phenomenon of learning consists in large part in the attaching of the object-adjustment intents to these stimuli.

We may now turn to the experimental evidence on these points.

(1) We may begin by recalling De Camp's and Sams' experiments. For these really give evidence not only about facts of 'purpose' but also about facts of 'cognition.' They indicated not merely that rats will *prefer*, other things being equal, the spatially and temporally shorter of two paths, but that they can also within certain limits *adjust* to (*impute*) the ones which are the spatially and temporally shorter. As a result of such experiments we may conclude therefore that the first important features of behavior-structure to which the rat acquires a cognition intent are just these of relative spatial and temporal extents. Whenever a given alley presents too great a distance or too great a time, it comes to be rejected for one which is shorter.

But are the above the only adjusted to features which maze learning involves? What about blind alleys? Are they also merely a matter of excess distance or excess time? And is the reason for their elimination merely the fact that the going in and out of them is adjusted to (cognized) as involving this excess distance and this excess time? That distance factors do play a part in their elimination is evident from certain experiments which have been tried out at California. Thus Mr. White and myself [1] found that, given two blinds (both relatively deep and both presenting the same sort of initial straight-away) the longer of these two was eliminated first. In other words, that which causes a blind to be elimi-

[1] Tolman, E. C., and White, A. E., *J. Comp. Psychol.*, 1923, 3, 327-331.

nated *is*, in part at least, its excess distance. For the greater the excess distance which a blind presents, the sooner it will be eliminated.[1] But this is not all. We have been performing another experiment at California. In this further experiment the animal was presented with a blind, with a true path, and with a roundabout route symmetrically placed as compared with the blind. Further, the blind was of a length such that the going in to it and the coming out again, plus the then taking of the true path, was of exactly the same length as going all the way by the roundabout route. Our results, as far as we have gathered them, indicate that in such a situation the blind is eliminated a *little before* the roundabout route involving the same total distance. Or, in other words, it appears that a blind, as such, *does* allow some additional type of object-adjustment (cognition) over and above that of its mere excess distance. The question next arises, of course, as to what this further feature is. Two possibilities suggest themselves. The first would be that the essence of this further feature of the behavior-structure of a blind is that it brings the animal back to its starting-point. The other would be that its essence is to be found in the fact that the blind-end brings the animal up 'on its haunches.' Which of these it is must be tested out. An experiment to do so would be quite simple. All that would be needed would be to present the animal with a blind and with an excess distance of the same length which went around in a circle and therefore, like the blind, brought the animal back to the starting-point. If, then, the blind were eliminated before this roundabout route, we could conclude that the distinctive thing about the to be cognized behavior-structure presented by a blind was something more than its bringing the animal back to the entrance, and presumably something about its bringing the animal to a stop and forcing retracing *per se*.

[1] It will be recalled that Peterson found that the shorter of two blind alleys was eliminated first. But both his blinds were, absolutely speaking, short and it seems probable that his animals were responding directly to immediate cues which would be more prominent in his shorter blinds than in his longer ones. His results, in short, probably indicated facts about perception rather than facts about learning. Peterson, Joseph, *Behavior Monog.*, 1915, 3.

(2) We turn now to the other point—the nature of the stimulus cues and of the laws governing the attachment of the cognition intents to them. Initially and before experience an animal cannot know (except in the case of relatively short blinds—i.e. the case of immediate perception—what the behavior-structure of a given alley is to be.

He must enter and find out whether it is long or short, or whether it is of the nature of a blind or of an unobstructed passage. And learning consists in attaching such resulting cognition intents (object-adjustments) to whatever distinctive stimulus-cues may be discoverable at the entrances. The more capable animal will be the one who can thus attach the cognition intent to earlier and earlier sensory-cues and make such adjustments quickly.

The immediate problem subdivides therefore into (*a*) a discussion of what distinctive stimulus-cues the particular organism is capable of attaching its cognition intents (object-adjustments) to; and (*b*) what are the governing circumstances for the acquisition of such attachments.

As regards (*a*) the literature is relatively extensive. The well-known work of Watson [1] on the apparent ability of animals to learn mazes with all but the kinesthetic cues eliminated falls under this head, as does also that of Miss Vincent [2] in showing that on occasion the animals nevertheless can be made to depend upon visual, olfactory or tactual cues, when these are emphasized.

As regards (*b*) discussion is still rife. Are the mere frequency and recency with which given stimulus-cues are followed by given behavior-structures the only factors determining the formation of associative connections from the former to the latter, or are other influences of an instinctive or emotional character also involved? Is, in short, the 'law of exercise' enough, or is the 'law of effect' also necessary? The evidence is still obscure and largely so, I believe, because the sort of analysis of the situation, which we are here recommending, has not ordinarily been made. Previous work, for

[1] Watson, J. B., PSYCHOL. MONOG., Ser. No. 33.

[2] Vincent, S. B., *J. Animal Behavior*, 1915, 5, 1–24, 140–157, 175–184, 367–374.

example, has made no distinction between an animal's mere *knowledge* of the behavior-possibilities which will get him to food and his *desire* for those behavior-possibilities. Whether an animal actually enters a given alley or not is probably a function, not merely of the growth of his *knowledge* about the nature of that alley but also of the state of his desires as we indicated in Section I. And experimental work must be done on tearing these two factors apart, before we can be certain to what extent the acquisition of mere knowledge, as such, is a function of mere frequency and recency or how much it depends also upon the satisfactoriness or unsatisfactoriness of the result.

Conclusion

First, it is to be emphasized that the primary object of this paper has been to demonstrate the *fruitfulness* of some sort of purposive (goal-seeking) and cognitive (object-adjustment) categories applied to learning. It has not been to insist that the particular purpose and cognition categories here suggested are necessarily the best that could be found.

Second, we wish to point out that in adopting such purpose and cognition categories as a substitute for the usual stock, physiological ones of synaptic resistance and conditioned reflex, we are not denying that the physiological concepts will ultimately prove the more comprehensive and accurate. Eventually we will undoubtedly have to reduce and explain our more immediate categories of goal-seeking and object-adjustment in terms of physiological categories. But the date at which this last will be possible is far distant. And, practically, it seems that the current tendency to talk and think primarily in terms of such inadequate and premature physiological concepts as are now on hand is in part responsible for some of the barrenness of our present animal research.

Finally, it is to be pointed out that the above discussion, in spite of its use of the terms *purpose* and *cognition* is *behavioristic*. For in no place in using these concepts have we defined them 'mentalistically.' We have not in some mysterious fashion 'looked within' and so discovered them. Rather have we looked without at the rat in the maze and

merely proceeded to describe the behavior which we saw. And the animal's cognitions (object-adjustments) and his purposes (goal-seekings) as we have observed them have been described and *defined* in purely *objective* terms; in terms, that is, of (*a*) the objective maze situation itself, and of (*b*) purely objective and descriptive aspects of the rat's behavior in the presence of that situation.

GENERAL ASPECTS OF THE CONDITIONED RESPONSE

BY HULSEY CASON

Syracuse University

The conditioned response is a fairly common process or event in animal life and a legitimate subject for psychological investigation. It is opposed to the concept of fixed structures and functions, and falls outside the realm of the strictly biological sciences. This principle of learning has already engaged the attention of a large number of psychologists with varied interests, and it is no longer the exclusive property of any particular school. The present paper describes some general characteristics of the conditioned response which are not commonly recognized. We consider certain reflexes and sensations as the hereditary modes of behavior, and attempt to explain the development of the more complicated mental phenomena on this simple basis. The general point of view may be briefly characterized as a monism of action.

Complications of the Conditioned Response

In Fig. 1, let us suppose that A is the adequate stimulus for calling out reaction A', and B the adequate stimulus for calling out B', and, furthermore, that either of these associ-

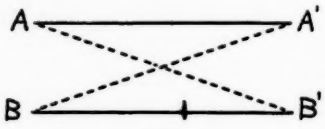

Fig. 1

ations may have been inherited or acquired. Since all parts of the nervous system are more or less intimately connected with all other parts, there is a possibility that the nerve fibers involved in the path A–A' will intermingle at some

points with the fibers involved in the path *B–B'*. Although *A–A'* may not lie in the direct path from *B* to *B'*, and the path from *B* to *B'* may not lie in the direct path from *A* to *A'*, it is practically certain that the neural activity in one path will have some effect on the other, however weak, remote, or indirect. As shown in Fig. 1, *two* different conditioned reactions may be established by the simultaneous activity of the two original paths,[1] and the acquired reaction-tendencies are represented in the figure by the dotted lines. The action of the system increases the number of responses which may be evoked by *A* or *B*, and at the same time and in the same way multiplies the number of adequate stimuli for *A'* or *B'*.

If we consider the connection *B–A'*, *B* may be considered as a substitute stimulus connected with *A'*, and *A'* as a substitute response connected with *B*. Thus the association *B–A'* is at the same time both a substitute stimulus and a substitute response. It is misleading to speak of an 'afferent modification' *or* an 'efferent modification,' because whenever one is changed the other is necessarily affected. As a matter of fact, the *modification* is neither in the afferent nor in the efferent neurons, but in the central connections. When learning takes place, the afferent fibers remain in the same position and continue to function in the same way, granted that they receive the same stimuli; and the disposition of the efferent fibers likewise remains unchanged. When an association is formed, some structures or functions in the organism are necessarily *modified*, but this change takes place in the *central nervous system*. The expression 'conditioned response' is seen to be misleading, since it does not place enough emphasis on the changed relation between the stimulus and the response. However, the response is after all the most significant aspect of behavior with which we are generally concerned, and from this point of view 'conditioned response' may not be such an unfortunate phrase. It should also be remembered that in forming the connection *B–A'* the substitution process is not entirely complete, since the *A'* called

[1] Something like this is suggested by Herrick, C. J., 'An Introduction to Neurology,' 1918, 61, 67–68, 269, 328–330.

out by B is not as strong and does not occur in as prompt a fashion as the A' called out by A. The considerations just noted in regard to B–A' apply in the same way to the connection A–B'.

To make the illustration more concrete, let A in Fig. 1 represent acid in the mouth of a dog (as in Pavlow's experiment), and A' the flow of saliva. B represents the bell stimulus, and B' the 'secondary reactions' ordinarily evoked in a dog by the sound of the bell. Theoretically, it is to be expected not only that the bell will become associated with the flow of saliva but also that the acid will become associated with the 'secondary reactions.' One of these connections is generally much stronger than the other, or at least it seems to be, and it almost invariably happens that one can be more easily observed.

If the two responses A' and B' are antagonistic, then both cannot be evoked at the same time because of the mechanical obstruction. This is what was observed in an experiment on the pupillary reflex, where a bell stimulus would dilate the pupil and a light would contract it at the beginning of the training, but later either stimulus would cause a pupillary contraction.[1] One must conclude therefore that native and acquired connections do not always manifest themselves in overt muscular responses; and the stimulus-response concept becomes fairly complicated.[2] In the present paper the term 'response' denotes any activity in the organism caused by a nervous impulse after the impulse has traversed the afferent neurons. A 'response' therefore may be some form of activity in the central nervous system, or in the effectors, or possibly in both. To limit the meaning of the term 'response' to the activity of 'effectors' would fail to take account of the numerous dispositions in the nervous system, which though definitely established do not find expression in overt movements.[3]

[1] Cason, H., 'The Conditioned Pupillary Reaction,' J. Exper. Psychol., 1922, 5, 108–146.

[2] See Hunter, W. S., 'The Symbolic Process,' Psychol. Rev., 1924, 31, 478–497.

[3] To illustrate: Suppose a child tries to connect '*Hund*' and 'dog' in a brief practice. Directly afterwards he may not be able to respond '*Hund*' to the stimulus 'dog.' No overt response can be made, and yet, some disposition has been left in the nervous system, where the response occurs; and the individual is not the same.

Although no controlled laboratory experiments have been carried out which demonstrate conclusively that *two* conditioned responses are always formed, there is some evidence which indicates that this may happen in the natural course of events. Thus after the first few days of life a moving object is an adequate stimulus for rotating the eyes or fixating; and moving objects frequently produce sounds which are of sufficient intensity to cause a reflex jump (Fig. 2).

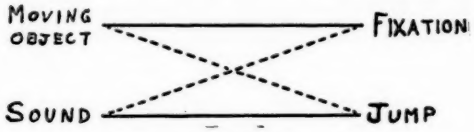

FIG. 2

The visual stimulus from a moving object may cause fixating, and the auditory stimulus from the same object may cause a start. After some training the visual impression of the moving object may cause a start, in addition to fixating; and the sound of the object may cause a reflex turning of the eyes, in addition to the start. Opportunity for such training is afforded when the child throws certain objects on the floor, when he strikes various objects to produce a sound, when something falls or is dropped accidentally. There is a possibility, of course, that these responses may be acquired in another way, and for other reasons, and our conditioned response principle may not be involved at all. However, the illustration has a certain degree of plausibility, and the conditions for establishing a conditioned response are very favorable when the same object produces two stimuli simultaneously on several occasions.[1]

The view that a conditioned reflex may be involuntary and independent of 'consciousness' has been criticized recently by Professor T. V. Moore.[2] The reflex theory, however, is supported by a good deal of evidence. The experiments on reaction-time have shown that the withdrawal movement of the finger and hand may become quite reflex and involuntary in nature. But this is an acquired

[1] See Stern, W., 'Psychology of Early Childhood,' 1924, 67–135.

[2] 'Dynamic Psychology,' 1924, 68–72.

XUM

reflex. The reflex theory is also not solely dependent upon the laboratory studies of reaction-time and conditioned response which have been made. Opening the mouth when food is raised to it at the table is certainly a learned act, but it occurs in an involuntary, automatic, and reflex fashion. Moving the eyes back and forth while following the lines of a printed page is also a reflex act evoked by and conditioned to a general situation. These facts do not give any encouragement to *materialism*, as a few writers on psychology have claimed, for some of our automatic activities are very useful and even *ideal* modes of behavior.[1]

The Mechanism of Redintegration

There is apparently a very close relation between the conditioned response and the redintegrative mechanism; in fact, if we are dealing with a number of conditioned responses functioning at almost the same time, the situation seems to be practically the same as that found in redintegration. This mechanism is dependent upon the fact that one stimulus may become associated with more than one response. Let us suppose that in the redintegrative mechanism (Fig. 3), stimuli

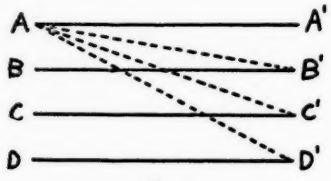

Fig. 3

A, *B*, *C*, and *D* originally call out responses *A'*, *B'*, *C'* and *D'*. The group of responses is sometimes referred to as the 'total reaction' in redintegration. According to the redintegrative hypothesis, any one of the four stimuli can later evoke the 'total reaction,' or at least a good part of it.

An illustration of this mode of behavior may be taken from army experiences. For example, soldiers are trained to stand straight, and they frequently march in a military

[1] See Bechterew, W., 'The General Principles of the Reflexology of Man.' 2d ed., 1923. Russian text.

fashion to the sound of music. After leaving the military service, the sound of martial music may cause an ex-service man not only to quicken his pace but also to adopt the soldierly attitude. Some of the emotional reactions which were formerly experienced in military parades and reviews may also return. In this way, one stimulus may evoke several responses, as represented in Fig. 3. A further illustration of this same matter is found in the ordinary experiences connected with dancing. The man holds his arms in a certain position, moves his feet in an irregular fashion around the floor, adopts a facial expression which is not entirely displeasing to his partner, and some pleasant emotional reactions may also be experienced with the rhythmical strains of the orchestra. The movements of the feet and the other attitudes may in time become quite involuntary and reflex in nature. On a later occasion, the sound of rag-time music may evoke a pleased expression in the practiced dancer, and if already standing he may begin to move about the floor, with his arms in the characteristic position, and he may even experience some of his former pleasures. Part of the same reaction may also be evoked in the absence of the music, at the mere sight of one's habitual dancing partner.[1] As in the previous illustration, one stimulus may now evoke more than one response, and the mechanism of redintegration and the group of conditioned responses seem to be identical. Redintegration, however, is generally such a complicated process that one may lose sight of the well-defined elements which constitute it. We return to this matter again when we discuss some more complicated processes.

The Acquisition of Images and Perceptions

We have now to discuss what some would consider to be the separate question of whether these learning mechanisms can be applied to the 'higher' forms of cerebration as well as

[1] The fact that some people who heartily detest rag-time music begin to like it after they have learned to dance, finds a ready explanation in these conditioned response mechanisms. The music which was formerly annoying becomes associated with a satisfying reaction. The 'satisfyingness' and 'annoyingness' therefore could not possibly be the cause of the new association, as the 'law of effect' would have it.

XUM

to the easily observable modes of response which we have been describing. The interpretations which may be made in the introspective field are especially liable to error, and yet there is apparently little reason for believing that the learning process functions differently when we acquire a perception and when we acquire a new motor act. Approximately the same sense organs, nerves, muscles, glands, and so on are involved in the one case as in the other, even if they may be involved in a different way.

In spite of this uncertainty therefore and realizing the possibility of error, let us return to a simple diagram, with two stimuli and two responses. In Fig. 4 a patch of yellow color

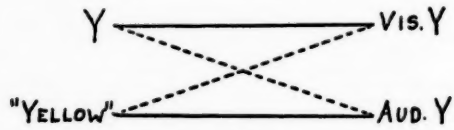

FIG. 4

evokes a sensation of yellow in a child, and the sound of the spoken word 'yellow' evokes the corresponding auditory sensation. It is not improbable that a child may simultaneously exercise these two connections on several occasions. In addition to the visual sensation called out by the yellow color, and the auditory sensation called out by the spoken word 'yellow,' the colored patch will later evoke an auditory response of the spoken word 'yellow,' and the spoken word will later evoke the visual response of the color.[1] It soon becomes impossible for the spoken word to call out a pure sensory response. The auditory stimulus 'yellow' becomes associated with other responses, and in the place of the pure auditory response of 'yellow' we have a 'pattern' or perception [2] of yellow, which may also include a visual response. The child perceives that the spoken word 'yellow' also means

[1] Perhaps we should ask the reader's indulgence for the monistic assumption and the disregard for some of the traditional psychology, which are evident in certain parts of this paper. We realize that monism is not yet entirely justified by scientific analysis, nor may it ever be,—but in this respect it does not differ from dualism.

[2] See Koffka, K., '*Die Grundlagen der Psychischen Entwicklung*,' 1921; Koffka, K., 'Perception: an Introduction to the *Gestalt-Theorie*,' PSYCHOL. BULL., 1922, **19**, 531–585.

something in another sense field; which is practically the same as saying that a perception is always a perception *of* an object or event.

The visual response of yellow which is called out by the auditory stimulus is not as strong or as constant as when it is called out by the patch of yellow. It is said to be faded, fleeting, washed out, because it does not have the advantage of the strong original stimulus. When a response in one sense field is evoked in this way through other pathways, it is ordinarily called an image.[1] In the same way, the yellow patch may later evoke a pattern which is made up of responses in both the visual and the auditory fields, but which is still a perception of yellow. When the auditory responses evoked by the yellow patch are particularly prominent, we are said to have an auditory image of this spoken word. If imagery is dependent upon the learning process, as we have supposed, one would not be surprised to find the large individual differences commonly observed. It would also follow that imagery could be cultivated to some extent in an adult person, which seems to be the case. Since vision and audition are used more than the other senses, one would likewise expect that visual and auditory imagery would be fairly prominent.

If an image resembles a fairly hazy and indefinite stimulus, it may be impossible to distinguish between this image and the perception (or sensation). This is true of adults in certain unusual circumstances,[2] and it has been frequently observed that a child cannot always distinguish between the 'real' and the imagined worlds.[3] This is probably due to the fact that a child's learning processes have not gone very far.

[1] Professor R. S. Woodworth writes that "The acquisition of mental images seems to be essentially the same process as the acquisition of conditioned reflexes." ('Psychology, a Study of Mental Life,' 1921, 403.) It is also suggested that the extra sensations in synesthesia "are images that have become firmly attached to their substitute stimuli during early childhood." (*Op. cit.*, 376.)

[2] Cason, H., 'Imagery in the Waking and Drowsy States,' *Amer. J. Psychol.*, 1923, 34, 486–495.

[3] See Stern, W., 'Psychology of Early Childhood,' 1924, 266–294.

Simultaneous and Successive Pattern Reactions

To make the general picture more typical of every-day experiences, let us substitute an orange for the patch of yellow color in the above illustration, and the spoken word 'orange' for the spoken word 'yellow' (Fig. 5). In addition

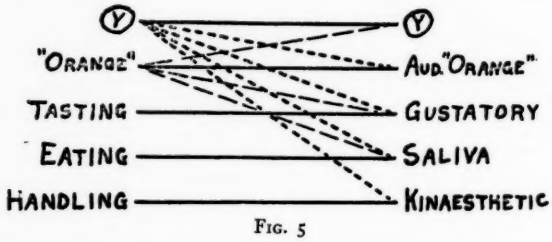

Fig. 5

to a visual response of color, the orange will also provoke a response which corresponds to its round shape. There may also be gustatory and salivary responses from tasting and eating the orange, and kinesthetic responses from handling it. If these connections are exercised several times, the sight of the orange may cause the child to think about the spoken word 'orange,' he may also experience a gustatory image of the orange, his saliva may begin to flow, and his hands even may move as if he were about to handle it. The spoken word 'orange' may call out an image which is both yellow and round, together with a gustatory image, accompanied by the flow of saliva, and so on. If the child is blindfolded, some of these same responses may be called out by putting the orange in his hand, or by putting a piece of it in his mouth.[1]

As in former illustrations, one stimulus may now call out several responses, and this group of conditioned responses seems to be the same as the redintegrative mechanism. The nature of the responses, however, may vary under different forms of stimulation, and these responses give meaning to the particular stimuli involved. In the first few years of life the child's responses have very little meaning for him. Disconnected experiences follow each other in rapid suc-

[1] See Smith, S. & Guthrie, E. R., 'General Psychology in Terms of Behavior,' 1921, 158–166.

cession, and he seems to feel very little discomfort at their general incoherence. One of the striking facts of developmental psychology is that the child has to learn practically everything, and in the first few years this learning is largely for present use.

The responses described above in connection with the orange constitute for the time being the child's ability to think about this object.[1] It is possible that more than one of the responses may be present or active simultaneously, although the child may not be 'aware' of all of them at the same time. If several responses are active simultaneously, then one response may become associated with one or more of the other responses. The nervous impulses may pass from various pattern reactions of muscles, glands, sense organs, or indirectly from most any bodily tissue, back into the nervous system.[2] By the same conditioned response principle, one response may later become the adequate stimulus for calling out some other response or some other responses. This process is probably the same as the associations of ideas or trains of images which we commonly experience.

Since thinking proceeds much more slowly than one would expect from the known speed of the nerve impulse, one is practically forced to conclude that a train of ideas is not a function of the nervous system alone. The time lost in traversing several synapses is also not sufficient to account for the discrepancy. Other data from nerve physiology support the same conclusion—and we may legitimately assume that the structures with which the nervous system is connected—sense organs, muscles, glands, etc.—are also involved in the thinking process. Thinking seems to be a function of the whole body,[3] and in a more limited sense it is also a function of the individual's total environment. According to Professor Dewey, "The reason a baby can know

[1] Cf. Givler, R. C., 'Psychology, the Science of Human Behavior,' 1922, 198–203, 321–322.

[2] See Kempf, E. J., 'The Autonomic Functions and the Personality,' 1921, 56–67.

[3] See Watson, J. B., 'The Place of Kinesthetic, Visceral, and Laryngeal Organization in Thinking,' PSYCHOL. REV., 1924, 31, 339–347, and other articles. See also Kempf, E. J., 'The Autonomic Functions and the Personality,' 1921.

XUM

little and an experienced adult know much when confronting the same things is not because the latter has a 'mind' which the former has not, but because one has already formed habits which the other has still to acquire." [1]

The above considerations suggest that there is no sharp line of distinction between thinking and doing. Thus in the illustration which we have been using, the spoken word 'orange' may evoke in the child an image of the shape and color of the orange, and this response may evoke an image of its taste. Any one of these responses may evoke a more overt movement connected with this situation. The responses shown in Fig. 5 may be evoked simultaneously or successively, in groups and patterns. It is obviously not necessary to suppose that a child can verbalize all of these experiences.[2]

Although we have been principally concerned with the case where one stimulus becomes associated with several responses, it is also significant that one response may become associated with a number of stimuli. On the stimulus-response hypothesis, the number of new stimuli and the number of new responses would be the same.

General Characteristics of Learning

Since many of the 'higher' forms of cerebration cannot be observed very accurately, some of these problems may continue to be mere matters of speculation. The child soon develops such a complex assortment of reaction-tendencies that it is impossible to know the origin of many of them; and the brief outline which we have given is by no means adequate to explain the origin of all types of conscious activity or conscious behavior. In the absence of precise knowledge, however, the question of how the varied types of reaction are acquired must be determined on the basis of some such analysis as we have made, and plausibility is a factor to be considered. With these limitations in mind, therefore, let us compare the conditioned response principle with some of the other theories of learning.

[1] Dewey, J., 'Human Nature and Conduct,' 1922, 182.

[2] See Watson, J. B., 'The Unverbalized in Human Behavior,' Psychol. Rev., 1924, 31, 273–280.

An important consideration here is that all of the acquisitions which we have described can be established by the conditioned response principle.[1] The so-called 'law of exercise' does not account for the formation of new associations. In a single stimulus-response connection at least, practice does not improve. The 'law of effect' is not a satisfactory explanation, partly because the 'satisfyingness' itself may be an acquired reaction. This hypothesis rests on the assumption of two mental 'faculties.'[2]

Although 'trial and error' is still referred to as a *means* of learning, it is apparently little more than a general allusion to the fact that an animal in a complicated situation will do first one thing and then another. As a phrase it is in the same class with 'random movements,' 'persistent activity,' 'active performance,' and the like. It describes some of the responses which the animal occasionally makes while learning is taking place, but it ignores the elementary situations which are probably responsible and necessary for learning. Closer study indicates that the 'trial and error' or 'trial and success' situation is generally so complicated that the individual stimuli and responses cannot be identified. If these are *unknown*, then it is practically impossible to be certain of the *mechanism of learning* which is involved. All that is known in most cases is that the animal has a drive of some sort. Just what the drive is from the scientific point of view and where it is directed, is frequently a matter of speculation. The conditioned response may be present, or it may not. No one can tell. In any event, little assistance will be obtained from attempting to explain the learning in terms of the 'successful' response. This type of explanation will be more useful when a description of 'success' in neural terms is forthcoming.

'Trial and error' is also quite inadequate as a *general description* of many forms of learning, and partly for this reason it cannot be considered as a general *law* of habit-

[1] A general review of the conditioned response literature, prepared by the writer, will appear in a current number of the PSYCHOLOGICAL BULLETIN.

[2] Cason, H., 'Criticisms of the Laws of Exercise and Effect,' PSYCHOL. REV., 1924, 31, 397–417; and Titchener, E. B., 'A Beginner's Psychology,' 1915, 84–86.

XUM

formation which will apply to all conditions. This inadequacy is evident in many every-day examples of learning. Recently I passed a cat in the yard several times and his only visible response was to look up from his sitting position and follow me with his eyes. On two later occasions I stopped long enough to stroke his back, and each time he stood up and arched himself in the characteristic fashion. Afterwards, he stood up and arched his back as I passed by, in the absence of the stroking stimulus. He would not make the same response to other persons. Now if this cat made any 'error' at all, it was to arch his back when I *failed* to touch him. His behavior can also be adequately described without any inference as to what he was 'trying' to do, if indeed he was 'trying' to do anything. In this illustration at least, the conditioned response is the most satisfactory explanation.[1]

If the main characteristics of the conditioned response are what we have supposed them to be, then negative adaptation should be a fairly general property of the nervous system. This seems to be the case. Repeat any stimulus over and over and the response connected with it will decrease in amount. This law is true not only for habits but also for the very limited number of inherited connections which have been identified. If a child looks at a book while you repeat the word 'book' several times, the spoken word may later call out a visual image of the book, and the object itself may later call out an auditory image of the spoken word.[2] Continued repetition, however, will soon cause the original sensations to degenerate, and the child's imaginative faculties will also in time cease to function. He will not look at the book indefinitely, nor will he continue to hear the spoken word.

The data on the so-called 'summation of stimuli' are not necessarily opposed to the view which we have presented. The summation effect can be observed in only a few exceptional cases, and in these cases it can be explained by the

[1] Cf. Wilson, W. R., "Selection in 'Trial and Error' Learning," PSYCHOL. REV., 1924, 31, 150–160.

[2] Cf. Strong, E. K., 'Introductory Psychology for Teachers,' 1919, 74–81.

operation of mechanical factors on the outside of the particular reaction arcs in question. For example, the continued repetition of a savory stimulus will increase the amount of saliva in the mouth, and eventually provoke swallowing. Although negative adaptation is a very general characteristic of the nervous system, and applies even to sensations such as pain, no summation effect can be observed in most of our activities. As far as we know, it cannot be observed in a single stimulus-response arc, and this is most important from our present point of view.

It may also be inferred from the above discussion that learning is never the result of activity in a *single* stimulus-response arc. It is important to recognize that an *additional factor* is always present.[1] The *auditory* stimulus 'book' and the corresponding *auditory* response cannot by themselves establish a *visual* response to this object. The visual and the auditory stimuli must be present at the same time.[2] This additional factor is recognized consciously and otherwise in the more complicated forms of learning by such expressions as 'will to learn,' 'attitude,' 'set,' 'interest,' 'concentration,' 'attention,' etc. In these cases, outside factors influence the particular stimulus-response arcs in question.[3] The well-known rule that a good way to memorize is to 'think about' the material in hand also illustrates the same general fact. It frequently happens in the natural course of events that the conditioned response situation is repeatedly exercised, and in such cases the associations may be very strongly established. Thus food in the mouth evokes saliva, and at the same time stimulates the olfactory receptors. Later the smell alone will evoke the saliva in a very regular fashion. As in a former illustration, one object acts as a double stimulus, and in such cases the conditions are very favorable for establishing a conditioned response.

In the more complicated types of habit-formation, such as

[1] See Kappers, C. U. A., 'Further Contributions on Neurobiotaxis,' *J. Comp. Neurol.*, 1917, 27, 261–298.

[2] For a description of the development of vocal expression through conditioned response, see Allport, F. H., 'Social Psychology,' 1924, 178–189.

[3] See Stern, W., 'Psychology of Early Childhood,' 1924, 259–262.

learning to play tennis, writing on a typewriter, or learning a language, so many responses are involved even in the first few minutes of practice that it is almost impossible to observe the specific nature of the process. At the present time no theory of learning can cover such complicated situations, and generalizations based on them are liable to be erroneous.[1] The above discussion has been confined for the most part to elementary matters, and we have not mentioned all of the factors which influence the strength of nervous connections. One should not expect too much from an elementary scheme of learning. It is probable that a simple mechanism of learning will not give any better picture of our every-day experiences in habit-formation than a simple description of an image will give an adequate account of our general capacities in imagination.

Redintegration and the Conditioned Response

Let us return to Fig. 3 and the discussion of redintegration. In this figure, stimuli *A*, *B*, *C*, and *D* originally call out responses *A'*, *B'*, *C'*, and *D'*; and there are several possibilities if various combinations are exercised simultaneously. If *A–A'* is repeated with *B–B'*, *A* may later evoke *B'*, in addition to *A'*. Also, if *A–A'* occurs frequently with *C–C'*, *A* in time may evoke *C'*, as well as *A'*. If *A–A'* and *D–D'* occur simultaneously, *A* may likewise become associated with *D'*. In case all four connections are active at the same time, it is clear that one stimulus may later evoke two or more responses, and this group of responses is the same as the 'total reaction' in the redintegrative mechanism. Any one of the responses in Fig. 3 may also become a stimulus, if it is not one already. Thus if responses *A'*, *B'*, *C'*, and *D'* are active simultaneously a number of times, response *A'* may later evoke responses *B'*, *C'*, and *D'*, by the conditioned response principle. These suggestions seem to cover all of the characteristics of the redintegrative mechanism which have been reported.

The view of the relation between redintegration and the conditioned response given above differs in several particulars

[1] This matter is discussed in an article by Mursell, J. L., 'The Principle of Integration in Objective Psychology.' *Amer. J. Psychol.*, 1924, 35, 1–15.

from that given by Professor H. L. Hollingworth. In the 'Psychology of Functional Neuroses,' Professor Hollingworth objects to the conditioned response as an aid in explaining the psychoneuroses principally because it is an old concept and because it has been used by the 'motor behaviorists.' He states that "As a simple illuminating concept it [the conditioned reaction] aptly describes many of the hysteric and neurasthenic symptoms," but he goes on to say that it is not possible to harmonize the conditioned reaction with psychology because of the 'Pavlowian atmosphere.' We read further that "It [the conditioned reaction] serves, in its present form, to describe the psychoneurotic picture in much the same fashion that the physician's hieroglyphic note to the pharmacist describes the very simple ingredients actually put into the bottle." [1] The psychoneurotic picture, however, is so complicated that it can be described neither by a single conditioned response nor even by a group of them. We realize that the redintegrative mechanism is of some general value in describing several psychoneurotic cases, but this fact itself does not discredit the data on the conditioned response, which were obtained for a different purpose. When Professor Hollingworth claims that the facts of the conditioned reflex were known to "the first savage who ever domesticated a beast of burden," [2] he underrates the value of the experimental data which are available.

Professor Hollingworth recognizes that the redintegration or 'total reaction' is made up of several part reactions, but he does not attempt to describe the way these part reactions are acquired. If the individual conditioned responses which constitute a redintegration, however, can be identified, we feel that it is justifiable and profitable to take account of them. In some cases the conditioned responses obviously cannot be identified, and while this is a limitation it is not necessarily a weakness of the conditioned response.

We are also unable to agree with Professor Hollingworth that *one* stimulus can evoke a '*total* reaction' which was previously aroused *in just the same way* by a group of stimuli.

[1] Hollingworth, H. L., 'The Psychology of Functional Neuroses,' 1920, 16–17.
[2] *Op. cit.*, 34.

No experimental evidence is at hand to indicate that the remaining stimuli would produce no effect at all on the 'total reaction.' Although it may seem that a single stimulus can evoke the same elements or types of response as the group of originally adequate stimuli, this assumption involves a fallacy which we think is of some theoretical importance. If stimuli *A* or *B* can evoke response *B'*, this response will be stronger for both of these stimuli acting together than for either one of them acting alone. This well-known dynamogenetic effect has been observed under a variety of conditions. One stimulus reinforces the other, and if the response is muscular more muscle fibers will be involved. If such is the case, then the response is not the same. We may also recall that the reaction evoked by a 'conditioned' stimulus is weaker than the same type of response evoked by an originally adequate stimulus. Thus, in the case of the dog, less saliva will flow for the bell than for the meat, and the bell will also not cause the saliva to flow for as long a period of time. These considerations suggest that the fundamental concept in the redintegration hypothesis is partly in error.

In an article on 'Particular Features of Meaning,' [1] Professor Hollingworth gives further objections to the view that a redintegration and a group of conditioned reactions are identical. "The conditioned reaction principle assumes that some first reaction is made to an original stimulus or event. This is the unconditioned response. To this response is then supplied another stimulus, by associating this latter with the primary stimulus. But in the redintegrative sequence no distinction of fundamental reaction and primary and secondary stimuli need be made." [2] We may suggest that it is not a question here of whether it is *necessary* to make such a distinction, for it is an obvious advantage to make it if it can be made. The situation in 'redintegration' frequently involves so many unknowns that it is not *possible* to make it. The redintegrative response, Professor Hollingworth holds, "may be instigated originally by all the details of a stimulating (antecedent) situation, by a synergy of various stimuli, for

[1] PSYCHOL. REV., 1924, 31, 348–368.
[2] *Op. cit.*, 352.

the very first time. Thereafter, the occurrence of one of the details provokes the total reaction. . . . Redintegrative sequences need not involve reflexes, in any acceptable sense of the term, nor is their production dependent upon frequent repetition." [1] While a complicated situation may not *be* a 'reflex,' all cases of redintegration seem to *involve* reflexes. All of the examples given in Professor Hollingworth's 'Psychology of Functional Neuroses' involve several reflexes. We would also question whether redintegrations are generally established with little training, whereas 'frequent repetition' is always necessary for a conditioned response. In many cases of redintegration continued repetition of a group of stimulus-response connections over a period of several weeks is apparently necessary; and a child can form a conditioned response to a flame or to a threatening gesture in a few seconds. There is apparently less difference between the conditioned response and the redintegrative mechanism than between redintegration and the mass of symptoms observed in the psychoneuroses. While this is a limitation of redintegration, the theory should not be judged on this basis alone. If a 'mechanism' can afford a fairly satisfactory explanation or description for just a few complicated situations, it may be a sign that considerable progress has been made.

Conclusions

1. The conditioned response is a common process or event in animal life, and as such it is a legitimate subject for psychological investigation. Such an investigation can be carried on more effectively from a monistic point of view.

2. If two stimulus-response arcs are active simultaneously on several occasions, there is a possibility that two conditioned responses may be formed, instead of only one. The conditioning takes place in the central nervous system and not in the afferent or efferent neurons. We have used the term 'response' to denote any activity caused by a nervous impulse which has already traversed the afferent neurons.

3. Images and perceptions may be acquired by the conditioned response principle.

[1] *Op. cit.*, 352–353.

4. Thinking is largely a function of habit, and is not limited to the activity of the central nervous system.

5. In many cases of habit-formation, the stimuli and responses are unknown, and the particular principle of learning involved is questionable. When the stimuli and responses can be identified, the conditioned response seems to have the advantage over the other theories of learning. Negative adaptation is a general property of the nervous system. Learning is never the result of activity in a single stimulus-response arc.

6. A redintegration is the same as a group of conditioned responses. Neither redintegration nor the conditioned response, however, can explain the complex mass of symptoms observed in the psychoneuroses. The conditioned response is more strongly supported by scientific data than redintegration, and is a more elementary form of behavior.

CONTRIBUTIONS TO THE PSYCHOLOGY OF NUTRITION

I. HUNGER AND APPETITE

BY JAMES L. MURSELL

Lawrence College

According to conceptions generally held, the taking of food depends upon two correlative but more or less independent factors, hunger and appetite. This view we owe largely to the work of Cannon, Carlson, and Washburn upon the activity of the empty stomach, supplementing the extensive researches of Pavlow. It is the purpose of the following paper to challenge this whole assumption in the interests of an entirely different theory, and to show that for the psychologist it presents such serious difficulties as to be untenable. Perhaps it may be worth while to remark at the outset that the view of hunger and appetite as correlative and independent factors in motivating ingestion has been put forward on the basis of physiological rather than psychological studies. Now the whole purpose of physiology is to analyze functions into their ultimate constituent organic processes, and thus it becomes very plausible to identify hunger with the peristaltic contraction of the fundus of the empty stomach, while, on the other hand, appetite is regarded as the whole mechanism of the psychic digestive secretions. There is, of course, neither question nor doubt that the two sets of processes actually take place, and that the contractions of the empty stomach produce the specific sensation known as the hunger pang, and regarded as constituting the 'call for food.' But to transfer these results directly over to the field of psychology, and claim that we have here discovered the two primary motives which condition and control the process of feeding, is entirely gratuitous. As a matter of fact a careful examination will show very serious difficulties in

XUM

the way of regarding either hunger or appetite as defined by the physiologists as a definite motive at all, while at the same time we shall find ample grounds for a far different and more satisfactory theory.

I. Gastric Hunger

That the distinctive sensation variously described as pain, pressure, emptiness, burning, gnawing, and so forth, and usually referred to the pit of the stomach, is caused by peristaltic contractions in the region of the fundus, has been established beyond any question. Direct investigation has established that the sensation is absent if the stomach is quiescent, that it begins almost immediately after the contractions have been initiated, and that its intensity is proportional to their strength. Moreover, it is the only account of many that have been proposed which is at all able to explain the various phenomena characteristic of the sensation of gastric hunger—its periodicity, its sudden onset, its equally sudden abolition, and its appearance in pathological cases, where the stomach is not yet empty. At the same time there remain over a whole set of problems of the highest importance which the researches producing the above results do not so much as touch, and which greatly complicate the whole psychology of nutrition.

1. To begin with the most highly specialized of these problems, complications are found when we inquire as to the action of acids upon the stomach. Cannon specifically asserts that hunger is not due to hydrochloric acid in the stomach, and Carlson says that "all acids, or liquids containing acids, including normal human gastric juice, cause inhibition of the movements and tonus of the empty stomach when introduced directly into the stomach cavity."[1] This is a summary of experimentation in which acid concentrations up to 0.5 per cent. were employed. These statements, however, appear directly to contradict a large mass of seemingly reliable clinical testimony. And Boring, using 5 c.c. of hydrochloric acid solution of 5 per cent. concentration finds a

[1] Carlson, J. C., 'The Control of Hunger in Health and Disease.'

very decided hunger pang set up.[1] It is most improbable that the stronger solution produced any facilitation upon peristalsis. Much more likely is it that here we are confronted with a complication quite outside the range of gastric hunger proper, and having little to do with any direct stimulation or inhibition of the muscular contractions of the walls of the stomach. Boring administered his dose by way of the mouth, and its taste, with the accompanying knowledge that it was acid, would certainly have far-reaching effects. This would harmonize well with the familiar action of bitters in producing a desire to eat, for according to Pavlow bitter drugs have no direct facilitating effect upon the secretion of the gastric glands, and according to Carlson they do not facilitate hunger contractions, yet both authors recognize their therapeutic value in stimulating ingestion, and this they attribute to psychic factors. In such cases, then, we have flavors, with their constellations of psychic associates, which are able not merely to increase secretions, but to initiate gastric contractions themselves, and whose influences are sufficiently powerful to overcome the direct effect upon the stomach tissues of the bitter or acid substances. Clearly then, normal gastric hunger becomes dependent on far more complex conditions than the mere emptiness of the stomach.

2. Another outstanding problem is the very distinct paradox of the rapid abolition of the peristaltic contractions upon the ingestion of even small quantities of food. For it directly follows from this that though hunger may initiate the process of eating, it cannot continue to motivate it once it is fairly started. In other words, we often begin a meal under one set of influences, and eat a few mouthfuls, after which we quite unconsciously switch over to the control and urgence of another set which carry us along without diminished momentum or changed experiential quality. If it were indeed the case that our entire trophic situation were revolutionized by the first mouthfuls of every meal begun out of hunger, it would surely be expected to leave some trace on the common experience of human beings, and to be

[1] Boring, E. G., 'Processes Referred to the Alimentary Tracts,' etc., PSYCHOL. REV., 1915, p. 306.

perceptible to introspection. But this is not so. All that we know would tend to make us believe that we continue well towards the end of a meal under the sway of much the same influences that initiated it. We strongly desire to eat, and when food is available we greedily attack it without being at all aware of any conspicuous change in the cœnesthetic and affective mass which constitutes the dominant undertone of our experience until well on towards complete satiety. At the same time we know with certainty that a profound change of condition has come about inasmuch as peristalsis has ceased long since. Clearly then, the question is fairly raised as to whether gastric hunger in the sense of peristaltic sensation ever acts as a direct motive for ingestion at all. It may well be that we actually take food under the urging of a highly complex set of influences among which gastric hunger is more or less prominent according to circumstances, but from which it can be eliminated without any very noticeable difference being registered in consciousness.

3. Still more serious is the problem presented by the well attested special 'hungers' for various substances. The best known of these is ordinary 'salt hunger' mentioned by Carlson himself. Zoölogy, anthropology, and clinical studies unite to attest the extraordinary persistence and urgency of the demand for sodium chloride. Desire to gain possession of this substance is one of the frequent causes of raids by savages on expeditions of white men; the eagerness with which the herbivora avail themselves of it is very well known; and epileptics subjected to a regime of dechlorination have been found, on escaping surveillance, literally gorging themselves on salt eaten by the handful. In the same way there is a great deal of evidence pointing towards the existence of a true special hunger for water. Thirst is usually supposed to be directly dependent upon the drying of the mucosa of the mouth and pharynx. But Boring's work would seem to point towards a more general condition underlying this specific and often artificially produced state. And Turro claims that thirst is set up by the general dehydration of the tissues, and that its intensity is proportional to the tension

of glucose and salt within the organism, inasmuch as it can be caused or eliminated by direct injections either of appropriate solutions or of plain water.[1] Turro also advances plausible arguments for specific hungers for protein, carbohydrates, fats, etc. While these contentions are by no means so well supported by experimental and clinical evidence as was the case with salt hunger, impressive confirmation is to be drawn from the field of comparative and historical dietetics. When we find widely sundered races, with vast differences in tastes and preferences, in food opportunities, and in habitat still keeping to pretty much the same general balance of rationing, it is difficult to avoid the impression that we are in the presence of a very powerful autoregulative mechanism, which, given certain external conditions, strongly favors a certain proportion of intake from among the basic nutrient substances. What this mechanism is, we frankly do not know. But there is very convincing evidence of its universal operation. The ordinary treatments of hunger and appetite offer absolutely no clue here at all. Turro's own suggestion of a 'trophic center' located somewhere in the basal ganglia hardly seems to be tenable, inasmuch as it is entirely unknown to neurology, and even as an hypothesis leaves many gaps.[2] But the great fact remains that somehow, with an enormous wastage due to individual error, the race has from age to age and everywhere managed to strike an average balance of diet not too extravagantly removed from what would be recommended by the best modern scientific opinion. Probably the best hypothesis covering these facts is that of certain positive chemotropisms which operate to set up cravings for specific substances. This view seems decidedly more tenable than that of Turro, who continually exaggerates the specificity of what he calls the 'trophic reflex,' and practically stands sponsor to the view that if the individual eats exactly what he likes he will come out just right in quantity and quality. The existence of general chemotropisms, on the other hand, would sufficiently

[1] Turro, R., 'Les origines de la connaissance.'

[2] Turro, R., 'Die physiologische Psychologie des Hungers,' *Zsch. f. Sinnespsychol.*, 1910 and 1911.

explain the very remarkable continuities which are revealed by long time averages, and at the same time account for individual variations. Here then we find a most important phyletic factor, operating as a sort of background to the nutritional behavior of every individual, which yet has no relations at all to either appetite or hunger as ordinarily understood.

4. Our final and in some respects most general problem in relation to gastric hunger may be stated in Carlson's own words: "Why does this feeling of uncomfortable tension or hunger pang in the stomach induce us to take food?" And once again we are brought face to face with a factor of determining importance in trophic behavior, which yet has nothing to do with either hunger or appetite. For the immediate reply to Carlson's question is that so far as original or innate connections go, gastric hunger never does, and probably seldom would, lead of itself to the taking of food. The hunger contractions are set up prior to the earliest postnatal food experiences, and they mean nothing more than a general unpleasantness to be allayed if possible. Conceivably an infant equipped with nothing else than this mechanism might learn the secret of eating to relieve the painful tensions before it was too late, and no doubt parental care would strongly tend to further such a result. But swallowing would have to be induced a good many times before the lesson would go home, and when we bear in mind that for an infant so equipped, to have something edible placed in its mouth would mean no more than to have such matter placed in its hand, it is clear that the proportion who would fail to learn would be high even under the best civilized conditions, and taking the entire human race, and still more so all mammalian young into account, the persistence of the higher forms of animal life would be barely conceivable. Fortunately, the young of all the higher vertebrates are also equipped with a mechanism for special and persistent buccal experimentation, namely, sucking, which is in fact the most highly organized of all the original repertory of reactions which bring the infant into contact with its environment.

XUM

The result is that we enter the world very well fitted to make rapid discoveries in the very realm where success means quieting the dominating claims of the stomach. Thus the resulting infantile skill in taking and swallowing appropriate food, which is the most primitive and the most essential of all human accomplishments, is the result of a learning process which connects together two reflex factors, sucking and peristalsis, both of which are about equal in their demands upon the organism. It is not the hunger pang itself, but the hunger pang organized by learning into a complex whole of behavior, that leads to the selection and ingestion of appropriate food.

II. Appetite

The physiologists recognize definitely enough that some factors other than gastric hunger are intimately involved in the business of taking food. Carlson says: "In the every-day life of adult persons having access to a liberal food supply, the memory elements of appetite are probably a greater factor in the ingestion and digestion of food than hunger, as short intervals between large meals do not permit the development of strong hunger unless the individual is engaged in hard physical work." [1] But when we ask for a precise analysis of this appetite factor we find considerable uncertainty. In dealing with hunger we were at least in the presence of an indubitable fact, that of the contractions of the empty stomach. In the case of appetite, however, the ground is much more shifting, for even the rôle of the psychic gastric secretion is not universally agreed upon. Our scrutiny here will reveal additional evidence of the psychological inadequacy of current views of nutrition, and point once more towards a new and different theory.

Pavlow's whole discussion of appetite centers about the psychic secretion of gastric juice as its most important element. As he puts it: "Appetite spells gastric juice." [2] His central interest, however, is to demonstrate the great importance of the psychic secretion for proper digestion; with the control of ingestion he is not so much concerned.

[1] Carlson, A. J., 'The Control of Hunger in Health and Disease, pp. 99–100.

[2] Pavlow, I. P., 'The Work of the Digestive Glands,' p. 95.

He explains at length how various mealtime conventions, such as taking food in moderately small mouthfuls as opposed to bolting it hastily, the social and esthetic surroundings and setting of the meal, the conventionalized consumption of alcohol, and so forth, have a great indirect value by touching off conditioned secretory reflexes. The therapeutic action of certain drugs, too, is attributed to the same cause. And the whole purport of his classic studies on the conditioning of the gastric and salivary reflexes is to show how beautifully and with what fine gradations the digestive glands can be brought to respond to originally indifferent food signs. His approach to the problem of appetite is fairly indicated by an analysis which he presents of what happens when food is taken under unfavorable conditions, as for instance in the midst of very great preoccupation with other matters, when "there is no appetite juice or 'igniting juice' or at best very little," and the whole digestive tract suffers a severe strain. Even in connection with digestion, Pavlow steadily tends to overrate the importance of the psychic secretions, presumably because at the time his earlier work was going on, the significance of the movements of the empty stomach was not understood. And the relation which his conception of the appetite mechanism bears to ingestion is decidedly problematic. For obviously the conditioned secretory reflexes of the digestive tract depend for their action, and certainly for their high selectivity, upon food experiences. It is only the familiar quality, the quality known to be pleasant and satisfying, that will produce the copious flow of gastric juice which Pavlow regards as the central characteristic of appetite. Untried and unfamiliar qualities produce no effect at all. Thus, in so far as appetite is indeed founded upon the conditioned reflexes of the digestive tract, it is an acquired rather than an original behavior, and an effect rather than a cause of food selections. No doubt we can say that appetite as understood by Pavlow gives us a partial explanation of food conservatism, for it shows how the whole organism tends to become geared to a determinate dietary regime, so that a change must involve a more or less complete gastric re-

education. But we have simply postponed the central problem which arises in connection with appetite, which is to explain how various food preferences come into existence in the first place.

Boring has some results which go to show that in a number of cases the various secretory reflexes may play a significant part in the determination of ingestion. Some of his subjects have reported a "kinesthesis of the throat, and oral sensations arising from the free flow of saliva, a complex which meant to them the desire for food." And his suggestion is that such a complex may constitute 'a true sensory basis of appetite.' There is, of course, no doubt that often where such sensory elements are present at all prominently they do actually integrate with the desire for food. But even this is by no means always the case. A drop of acid on the tongue will produce most of these phenomena, but it seems to have little to do with appetite. And again, these factors allow no qualitative variations and can differ only in intensity, and appetite, as usually distinguished from general gastric hunger, is a specific desire for certain foods. It would seem, then, that kinesthesis of the throat, and the sensations of profuse salivation, while they are frequent concomitants of the desire for food, are not properly speaking elements in the appetite complex itself.

Cannon puts forward a view of appetite which dispenses entirely with any sensory basis, visceral or otherwise, and rests his whole case upon memory and imagination. "Careful observation indicates that appetite is related to previous sensations of taste and smell of food." [1] Simple and plausible as such a suggestion may appear, it will hardly bear close analysis, for the underlying assumption is that there are certain tastes and odors that are pleasant or unpleasant *per se*, and that function in memory and imagination to produce attractions and repulsions towards certain food stuffs which possess them. But all that we know of comparative dietetics warns us against any such hasty generalization, and there is little to confirm it in the meager and dubious

[1] Cannon, W. B., 'Bodily Changes in Pain,' etc.

XUM

results of the psychological analyses of the gustatory qualities. What is extremely striking is not the uniformity and limitation, but the great diversity of gustatory qualities that are tolerated and enjoyed. Not considering the extremes of the scale of intensities, where the sensation practically becomes one of pain, and where sweet, sour, salt, and bitter, are all equally disagreeable, we find an immense range within which it is possible to learn to like or dislike almost any given combination of taste, odor, texture, temperature, and color in food. Any attempt to explain the food preferences of humanity on the basis of inherently pleasant or unpleasant tastes and odors is doomed to break down hopelessly, and in fact it is far truer that we like what we eat than that we eat what we like. It is notorious that uneducated taste is the most misleading of guides, and easily permits us to starve in the midst of plenty, or to eat noxious and poisonous substances. So the whole problem of nutritional education turns on training the individual to enjoy the right things, a matter of no small difficulty in a civilization like our own, where a bewildering dietary profusion of the most unequal values is provided for almost every one.

Carlson seems to come nearest to a really adequate analysis of appetite, and certain inconsistencies which appear between certain of his works, due no doubt to changes in his views and emphasis, seem on the whole to strengthen rather than weaken his presentation. Thus we find that he protests against the conception that clearly underlies all the associationist theories of appetite, that "appetite requires a nervous organization capable of associative memory," on the ground that we have "in appetite for food conditions as primitive and essentially fixed as in the case of the sexual desires or 'instinct.'" Thus appetite becomes "the expression of an inherited mechanism," which leads us to prefer certain foodstuffs.[1] This would seem in a measure to parallel the ideas put forward by Turro, who looks upon all vertebrates as manifesting definite hungers or desires for certain specific foods. For Carlson, then, the general drive to eat is consti-

[1] Carlson, A. J., 'Contributions to the Physiology of the Stomach,' *Amer. J. Physiol.*, 31, 175.

tuted by gastric hunger arising out of the contractions of the empty stomach, while appetite, or the desire for this or that special food, is an innate factor which probably should be classed as a chemotropism. In another work he puts forward a more synoptic view with a different emphasis.[1] On the basis of the work of Pavlow, Cannon, and Boring, he now suggests that the desire for food, in so far as it does not arise from the gastric hunger pang, is determined by a combination of visceral sensations arising from profuse salivation, together with the imaginal or sensory presence of pleasing food signs, the most important of which are tastes and smells. Both of these two elements must be present, and must be organized in terms of 'an approximately normal state of central correlations,' because the taste and smell even of the best of food may, in certain circumstances produce not appetite but nausea, because profuse salivation, when for instance it is produced by injections of pilocarpin, may have nothing to do with appetite, and because an improper combination of these two elements, even though both are strongly present, may give rise to no desire to eat, as in cases of nervous anorexia. While objections urged against the views of Cannon apply also to Carlson's formulations inasmuch as it seems once more to be assumed that certain gustatory qualities are pleasant or unpleasant in and of themselves, nevertheless Carlson, in recognizing the evidence for an original chemotropic mechanism, and in insisting that appetite must be a very complex factor psychologically, has carried the discussion further than any one else, even though we cannot regard his treatment as final.

III. Trophic Education

Our discussion of hunger and appetite has a twofold result. On the one hand, it has brought to light a number of unsolved problems in connection with the control of ingestion, and thus indicates the psychological inadequacy of the physiological concepts commonly employed. On the other hand, it has called to our attention the basic elements of the ingestive process. We are now to see how a fusion of these

[1] 'The Control of Hunger in Health and Disease.'

XUM

elements by means of trophic education gives the individual the food skills which are essential for life.

The first of the basic elements that regulate ingestion is sucking, the most characteristic, and innately the most highly organized of all the original external reactions of mammalian young. Watson comments specially on the surprising accuracy and quickness with which an infant only a few days old will move its head so as to bring the lips in contact with a finger lightly touching its chin or cheek.[1] This contrasts in the most striking manner with the slowness and vagueness of all the rest of its 'instinctive' repertory, excepting only such simple reactions as the grasping and plantar reflexes. In employing the stimulus mentioned, however, Watson's work is somewhat misleading, because it entirely misses one of the most important characteristics of infantile sucking, namely, its non-specificity with regard to its stimulus. It is found that almost any mild stimulation of the exteroceptors, as for instance the exhibition of a light, or the stroking of skin surfaces remote from the mouth, will set up this response. And furthermore, there is reliable evidence directly contradicting Watson's view that sucking depends definitely on physiological condition, for it has been found that gentle stimulation will produce it even immediately after a full meal. Some observers indeed go so far as to claim that sucking is a general sign of euphoria, and that the baby strongly tends to suck whenever it is pleased.[2] Thus we may think of the infant as a mechanism specially geared to commence sucking on almost any provocation. At first the response has absolutely no connection with food, and in fact it begins some time before the first feeding. But its meaning and utility is perfectly clear, for by this means the infant rapidly acquires a large fund of buccal experience, at first quite undifferentiated. Anything and everything is placed in the mouth and if possible swallowed. It is in this way that trophic education is commenced.

[1] Watson, John B., 'Psychology.'

[2] Boris Sidis, 'An Experimental Study of Sleep.' H. von Hug-Hellmuth, 'A Study of the Mental Life of the Child.'

Concomitantly with sucking, and appearing very early in life and before the earliest feeding we find the gastric hunger contractions. We have already noted that the sensations arising from them cannot at first constitute any call for food. But they are accompanied by a phenomenon characteristic of hunger all the way up and down the phyletic scale, to which Carlson calls special attention, namely, dynamogenesis, which in man is probably partly hormonic and partly neural, and which some few observations would seem to indicate as having a specially marked effect upon sucking. The situation then is this: The new-born infant has a general tendency to place objects in its mouth, and to swallow them; and it rapidly comes to learn that this action will allay the unpleasant hunger pang. Thus a connection is set up between the two sets of reactions, and hunger becomes not merely cœnesthetic malaise, but a call to eat. This, in fact, is the earliest piece of learning we ever perform, and the most important for individual survival. It is essential to note that here we have an acquired, and not an instinctive connection.

So far, however, we have come upon no mechanism by which food preferences are set up. In fact theoretically, if this kind of connection between sucking and gastric hunger were the sum total of trophic education, we would have no account even of how *edibilia* are selected in preference to other substances, inasmuch as any substance, digestible or not, will serve to some degree to check the contractions of the empty stomach. We have, however, two related influences powerfully at work to modify the course of learning, so that the original strong tendency to suck anything and everything tends to be more or less strictly limited to a sucking, chewing, and swallowing of articles appropriate for food.

The first of these influences is that of specific positive chemotropism for certain substances. This registers most manifestly by way of the chemical senses, taste and smell, so that various combinations of flavor and odor particularly when associated with the sensation of something in the mouth and the kinesthetic sensations of eating have power to

XUM

produce pleasure or displeasure ranging all the way from enthusiastic delight to physical nausea. It is, however, by no means implied that certain gustatory qualities are inherently pleasant or unpleasant. Although there is a wide margin of error, still the odor-taste combinations that are enjoyed are those of substances which are needed to restore or perpetuate a proper biochemical balance. This is Turro's thesis, put forward on the basis of a mass of comparative and statistical data as the only explanation of the wide uniformities of the dietary regime, and the remarkable integration of food habits with environment. It explains, for instance, why the child with its proportionately very large radiating surfaces, and its high energy expenditures, craves sweet-tasting substances which in effect mean the sugars, whose energy content is very high; it explains the preference of northern peoples for a high calory diet rich in fats while inhabitants of the tropics prefer a low calory ration. The mouth and nose are the chemical laboratories of the body, where a primitive but far from ineffective analysis is carried on. That process may of course result in error, but on the whole it is accurate enough. And its results are expressed in likes or dislikes whose average tendency is towards needed and superfluous substances respectively. If the flavors of all the *edibilia* were changed, there is reason to suppose that our tastes themselves would change. As Turro puts it, if lactose were bitter instead of sweet, the infant would crave a bitter taste and reject a sweet one.

Our liking or disliking for certain substances, however, is a great deal more than merely an affair of gustatory pleasure or pain. In the presence of a food for which there is a strong biochemical need, and consequently a strong craving, the whole organism becomes attuned to the situation. The appropriate and finely graded secretory responses are called into action, and the entire digestive tract prepares itself for the assimilation of this particular material. Conversely, if we are confronted with the probability of being compelled to eat some entirely superfluous or harmful substance, the secretory responses are inhibited, and disgust may go as far as

anti-peristalsis and vomiting. This explains one of the phenomena of learning which seems to have escaped extended comment, namely, the very much greater speed and ease of conditioning the digestive as compared to the motor reflexes. While so far as I am aware, carefully collated figures are unavailable, a comparison of Pavlow's work on dogs, rapidly and certainly culminating in the establishment of remarkably permanent psycho-reflexes, with Cason's studies of the pupillary and palpebral reflexes, which involves tedious and endless repetition and finally unstable results, cannot fail to be impressive. And the reason which is here suggested is that in the case of the digestive secretory reflexes, the stage is already set for a process of unconscious learning, for the whole organism is prepared by nature to respond selectively and accurately to a very wide variety of food signs. So when we speak of a positive chemotropism, we mean a very fundamental type of behavior involving the whole organic mechanism, which responds *en masse*, and with great readiness, to direct chemical signs or their associates. And it is in this way that sucking and gastric hunger become integrated in terms of a progressively more and more selective interest in appropriate foods.

The second influence that modifies and renders more specific the original generalized sucking tendency is that of convention, which, of course, is ultimately based upon chemotropism, and cannot depart very far from the limits laid down by biochemical need, and which, when ideally 'scientific' and enlightened, does not depart from those limits at all. Convention makes its first impression through the medium of parental care, which provides the foods supposed to be proper, prepared in some regular manner. Its work is carried on by the imposition of meal times and meal customs, and by culinary habits which are remarkable in their national distinctiveness. It serves to reinforce the selective effect of simple chemotropism in rendering certain flavors and odors pleasant or repugnant, and to these it adds an enormous range of food signs. Texture becomes a consideration; temperature too must be right, so that ordinarily soup is

XUM

eaten hot, raw oysters cold, and so forth; visual qualities become exceedingly important, and food must be properly served; and finally the social and material surroundings of the meal are welded into the unity and become an essential part of the drive to eat. The vast influence of convention superimposed upon a basis of chemotropisms is seen in the differing food behaviors of the greedy small boy and the epicure whose appetite is spoiled by being handed a used napkin. And the wide differences of taste among different peoples, some of whom insist on mixing their tea with rancid butter, while others must have their eggs black with decay, some demanding their game high, others enjoying living grubs as a luxury, and so on to endless instances, bear witness to the flexibility within determinate limits of gastric education.

Our conclusion then is that man's desire for food is not the result of one or two factors working more or less in isolation, but that it represents a complex and highly integrated mass of behavior involving many elements. It has its origin in the learned connection between sucking and gastric hunger. By degrees, due partly to nature, partly to nurture, the stimuli which lead to sucking, swallowing, and effective digestion are more and more delicately differentiated. And concomitantly, feeding becomes more and more dependent upon the incidence of the proper time, place, and circumstances, and more and more removed from the two primitive impulses to put something into the mouth, and into the stomach. Like every other highly organized piece of behavior, feeding becomes increasingly capable of functioning in the absence of its original simple impulsive preconditions. Actual gastric hunger may or may not be present, and under the normal conditions of civilized life it does not make much difference either way. For gastric hunger has become only a part of a very intricate and relatively self-sufficient whole. To revert to one of our early problems, this would seem to explain why we go on with a meal serenely unconscious of the elimination of stomachic contractions with the first few mouthfuls. Even the baby, whose trophic behavior is very rudimentary, is carried smoothly on beyond the point at

which hunger proper is inhibited, by the other factors in the situation—the tendency to suck, the general impulsions due to biochemical need, and the incidence of the nipple on the mouth. And above all, it becomes clear that we cannot separate out appetite as the correlate of hunger, and claim that food taking depends on these two independent factors. The drive for food is itself a unit in behavior, depending to be sure on the factors which physiological research has isolated, but more than each and all of them, inasmuch as it is constituted by their living integration.

XUM

DISCUSSION

THE SERIES OF BLACKS, GRAYS, AND WHITES

In a recent article[1] in support of the Ladd-Franklin theory of vision, Neifeld has pointed out that black is made an essential part of only one theory, that of Hering. Even here it has given rise to difficulties and to discussions which according to Neifeld are 'very inconsequential.' The Ladd-Franklin theory is invoked as giving the only adequate account of the black sensation. The Hering theory has undergone various modifications and alterations, as have all other theories, when new facts have been brought forward. Whatever our hypothesis of the physiological mechanism of the optic organ may be, it must be in accord with the psychological observations it is designed to explain. G. E. Müller's observations[2] that led to the positing of a cortical gray cleared up some of the difficulties raised by the black-white series, but did not dispense with the anomalous character of that series. The fact that these two colors seem at the same time to behave as antagonists and yet to mix to give intermediates, has for a long time bothered psychologists who have been interested in the elementary facts of vision.

The argument over the question, "Is black a sensation?" has been settled,[3] as Neifeld points out, in the affirmative. "To speak of black as the negation of all sensation, as does James Ward, is naïvely to assume a relation of identity between cause and effect—a pure fallacy." So far we are in agreement, but to say that "black is the result of the complete absence of stimulation," or that "the sensation of black is a definite sensation attached to a cortical situation of inactivity or rest," is to make the same naïve assumption. A stimulus whose physical value is zero is not equivalent to zero *stimulation* or excitation. Total absence of light does not necessarily give rise to the sensation black. The temporal and spatial factors play an essential part. Black can be seen only

[1] Psychol. Rev., 31, 498.

[2] '*Zur Psychophysik der Gesichtsempfindung*,' *Zsch. f. Psychol. u. Physiol. d. Sinnes.*, 10, 32 *f.*

[3] Ward, James, *Brit. J. Psychol.*, 1, 407 *ff.*; 8, 212 *ff.*
Titchener, E. B., *J. Phil. Psychol. & Sci. Methods*, 13, 113 *ff.*; 649 *ff.*

in the presence, temporal or spatial, of light. Zero stimulus, except in immediate spatial and temporal contiguity to other positive stimulation gives rise to the sensation gray which has been attributed to endogenous retinal or cortical processes. Zero stimulus arouses the sensation black only when it is preceded by, or when it is adjacent to white stimulation. But zero stimulus, when it is preceded by or is adjacent to red stimulation, arouses the sensation green. It is just as correct to say that green may be the result of zero stimulus as to say that black is the result of such a stimulus. The fact that other stimulation also will arouse green in no way impairs the argument. Thus the single intensity of blackness which should be correlated with the single intensity of zero fails to stand.

The difficulties with the Hering conception of the black-white series as modified by Müller still remain. That series distorts the symmetry of the theory. The 'cortical,' 'central,' or 'neutral' gray of Müller, however, needs no further justification. Observation has established it as a fact rather than as assumption. The difficulty lies in Müller's interpretation of the source and nature of this gray. Along with other workers in the field of vision he has fallen prey to the fallacy of assuming a one-to-one correlation between the single linear series of changes in the stimulus—intensity of light—and the concomitant visual sensations. On this assumption, black-gray-white is a single serial change of the same order throughout, decreasing black and increasing white with a constant addition of gray; and cortical gray is the intermediate between black and white and must therefore be made up of black and white in equal proportions. It is at this point that a question of fact was settled theoretically when the answer could have been given by a simple observation. If one begins with a pure white and adds black to it by small steps one does *not* obtain, as has always been assumed, a series of changes analogous to that obtained by adding yellow to red in a similar manner.[1] In the latter case the likeness to red remains present, though in diminishing degrees, throughout the series to the end point which is all yellow. In the case of black and white, however, whitelikeness does not appear at the first step away from black, nor until the series has been gone half through. When it does appear all blacklikeness has already disappeared. And the region between the disappearance of black and the appearance of white is occupied by a gray which is neither black nor white but only gray. The situation is the same as that

[1] Dimmick, F. L., *Amer. J. Psychol.*, **31**, 301 *f.*

which occurs when blue is added to yellow, or green to red. Gray has been mistaken for an intermediate between black and white, as we have pointed out, because of the assumption of a sensation series parallel to the stimulus intensity series, and because of the 'neutrality' and lack of insistence of gray. Once the uniqueness of the quality gray is pointed out, it is obvious that gray is no more an intermediate between black and white than it is between blue and yellow or between red and green. A critical attempt to find a blacklikeness and a whitelikeness in neutral gray fails. More obvious still to even casual observation is the absence of all blacklikeness in the light grays and of whitelikeness in the dark grays.

With this modification, the arrangement of the colors about gray becomes totally symmetrical. The only problem left for the Hering theory to solve is a purely physiological one—that of the nature and action of the 'reversible photochemical processes' of Müller, or of any other physiological mechanism that will not distort the observed psychological facts.

FORREST L. DIMMICK

UNIVERSITY OF MICHIGAN